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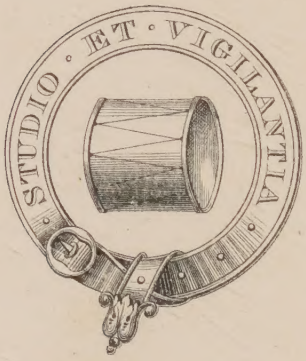

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OF

LONDON.

VOLUME THE TWENTY-THIRD.

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JAMES CALVERT, M.D., F.R.C.P.,

AND

FREDERICK C. WALLIS, F.R.C.S.

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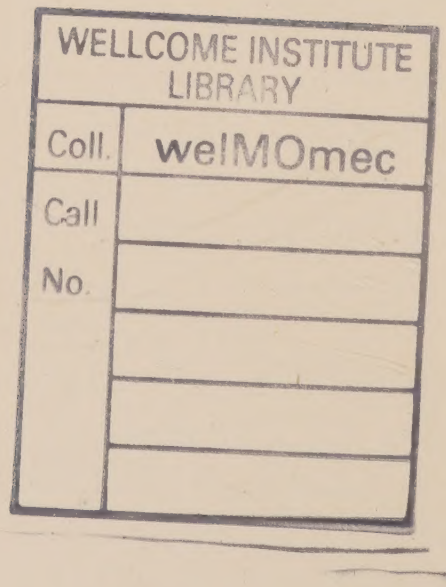
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1827. William Kingdom.
1828. John Burne, M.D.
1829. William Greville Jones.
1830. Leonard Stewart, M.D.
1831. Montague Gossett.
1832. John Whiting, M.D.
1833. Frederick Salmon.
1834. William Shearman, M.D.
1835. Walter Cooper Dendy.
1836. William F. Blicke, M.D.
1837. Edward Headland.
1838. Theophilus Thompson, M.D., F.R.S.
1839. George Pilcher.
1840. James Risdon Bennett, M.D.
1841. Wm. Dingle Chowne, M.D.
1842. Henry Hancock.
1843. Leonard Stewart, M.D.
1844. Thomas Bell, F.R.S.
1845. Marshall Hall, M.D.
1846. John Bishop, F.R.S.
1847. Golding Bird, M.D., F.R.S.
1848. Francis Hird.
1849. William Hughes Willshire, M.D.
1850. Francis Hird.
1851. Richard Rowland.
1852. Edwin Canton.
1853. John Snow, M.D.

1854. Henry Smith.
1855. James Fernandez Clarke.
1856. Benj. Ward Richardson, M.D., F.R.S.
1857. William Adams.
1858. Alfred Baring Garrod, M.D.
1859. Charles Henry Felix Routh, M.D.
1860. John Gay.
1861. Arthur Leared, M.D.
1862. Victor de Méric.
1863. Samuel Osborne Habershon, M.D.
1864. John Louis William Thudichum, M.D.
1865. Robert Greenhalgh, M.D.
1866. Thomas Christopher Weeden Cooke.
1867. Frederick William Headland, M.D.
1868. William Frederick Teevan.
1869. George Duncan Gibb, M.D.
1870. Francis Mason.
1871. William Cholmeley, M.D.
1872. Frederick James Gant.
1873. John Cockle, M.D.
1874. Robert Brudenell Carter.
1875. George Buchanan, M.D.
1876. Erasmus Wilson, F.R.S.
1877. John Hughlings Jackson, M.D., F.R.S.
1878. Alfred Carpenter, M.D.
1879. Walter John Coulson.
1880. William Henry Broadbent, M.D.
1881. Arthur Edward Durham.
1882. Edmund Symes Thompson, M.D.
1883. Edward Lund.
1884. Charles Theodore Williams, M.D.
1885. George Murray Humphry, M.D., F.R.S.
1886. Richard Douglas Powell, M.D.
1887. Sir William MacCormac.
1888. Sir Joseph Fayrer, K.C.S.I., M.D., F.R.S.
1889. Jonathan Hutchinson, F.R.S.
1890. Arthur Ernest Sansom, M.D.
1891. Sir Joseph Lister, Bart., F.R.S.
1892. Sir James Crichton-Browne, M.D., F.R.S.
1893. W. Mitchell Banks.
1894. William M. Ord, M.D.
1895. A. Pearce Gould.
1896. William H. Allchin, M.D.
1897. Edmund Owen.
1898. Frederick T. Roberts, M.D.
1899. Alban Doran.
1900. J. Kingston Fowler, M.D.

THE FOTHERGILLIAN GOLD MEDALLISTS.

1787. William Falconer, M.D.
1790. Robert Willan, M.D.
1791. John Coakley Lettsom, M.D.
1795. John Mason Good.
1801. Francis Bouttatz, M.D.
1803. Edward Jenner, M.D.
1824. Robert W. Bampfield.
1828. John George Parry.
1831. William Augustus Guy.
1834. William James Clement.
1835. George Moore.
1836. Thomas Egerton Bryant.
1838. George Pilcher.
1840. Samuel Osborn.
1842. James Risdon Bennett, M.D.
1843. John Weaver Lever, M.D.
1844. Henry Pratt Robarts.
1845. Walter Cooper Dendy.
1846. Robert Mortimer Glover, M.D.
1847. Silas Stedman.
1849. John Milligan.
1850. Richard Payne Cotton, M.D.
1851. Richard Hodges.
1852. Frederick William Headland.
1853. Alfred William Poland.
1854. Benjamin Ward Richardson, M.D.
1856. William Burke Ryan.
1857. Edwin Canton.
1858. Thomas Herbert Barker, M.D.
1859. Alderman Thomas Houghton Waters.
1868. John Clay.
1870. Thos. Smith Clouston, M.D.
1872. Edwards Crisp, M.D.
1873. John Kent Spender, M.D.
1877. Peter Murray Braidwood, M.D.
1878. John Milner Fothergill, M.D.
1882. Thomas Michael Dolan, M.D.
1883. Norman Porritt.
1886. John Strahan.
1888. Hobart Amory Hare, M.D., U.S.A.
1893.* William Richard Gowers, M.D., F.R.S.
1896. Victor Horsley, F.R.S.
1899. Sydney Monckton Copeman, M.D.

* *First Triennial Award under the Charity Commissioners' New Scheme,* vide *Trans., vol. xiv, p. xi.*

THE HONORARY FELLOWS.

1893. Barnes, Robert, M.D., Bernersmede, Eastbourne, LL, C.

1896. Berger, Paul, 16, Rue de Bourgogne, Paris. Professor of Clinical Surgery in the Faculty of Medicine, Member of the Academy of Medicine, Surgeon to "l'hôpital de la Pitié."

1897. Bergmann, Prof. E. von, M.D., Berlin.

1881. Billings, John S., M.D., Washington, Surgeon to the United States Army.

1899. Brouardel, Paul Camille Hippolyte, 85, Boulevard, St. Germains, Paris. Professor of Medical Jurisprudence, Dean of the Faculty of Medicine, University of Paris; Member of the Institute and of the Academy of Medicine.

1894. Catrin, Louis, M.D., Médecin-major de première classe, Professeur agrégé à l'Ecole du Val-de-Grace, Paris.

1873. Chauveau, A., late Professor of Physiology at the Medical School of Lyons, 10, Avenue Jules Janin, Paris.

1881. Emmet, Thomas Addis, M.D., 89, Madison-avenue, Surgeon to the Woman's Hospital of the State of New York.

1899. Fayrer, Sir Joseph, Bart., K.C.S.I., LL.D., M.D., F.R.S., 16, Devonshire-street, Portland-place, W. P, VP, LL, SM, O, C.

1886. Gairdner, Sir William Tennant, K.C.B., M.D., LL.D. Edin., F.R.C.P. Edin., F.R.S., 225, St. Vincent-street, Glasgow.

1894. Gant, Frederick James, F.R.C.S., 16, Connaught-square, W., Consulting Surgeon to the Royal Free Hospital. P, VP 2, LL, O, C 3.

1897. Guyon, Felix, M.D., Professor in the Faculty of Medicine, Member of the Institute and of the Academy of Medicine, Paris.

1897. Hingston, Sir W. Hales, M.D., Montreal.

1890. Kocher, Theodor, Professor, Berne.

1883. Le Roy de Mericourt, A., M.D., Paris.

1878. Mitchell, S. Weir, M.D., Walnut-street, Philadelphia.

1875. Ollier, Professor, Lyons.

1899. Osler, William, M.D., F.R.S., Baltimore, U.S.A., Professor of Medicine, John Hopkins University, and Physician-in-Chief, John Hopkins Hospital.

1899. Pachoutin, Victor Vasilivitch, 6, Nijni Grodskia, St. Petersburg, President of the Imperial Military Medical Academy and of the Medical Council.

1899. Potain, Carl Edouard, 250, Boulevard St. Germains, Paris; Professor of the Faculty of Medicine, University of Paris; Member of the Academy of Medicine and of the Academy of Science; Physician to "l'hôpital de la Charité."

1899. Rauchfuss, Charles Andreivitch, 8, Ligovka, St. Petersburg, Director of the Prince of Oldenburg's Hospital for Children.

1897. Roddick, T. G., M.D., 80, Union-avenue, Montreal.

1877. Sanné, A., 33, Rue de Naples, Paris.

1899. Sklifosoffsky, Nicholas Vasilivitch, M.D., Director and Professor of Surgery, Imperial Clinical Institute of the Princess Helena, Paulovna, St. Petersburg.

1873. Virchow, Rudolph, M.D., Professor of Pathological Anatomy in the University of Berlin.

CORRESPONDING FELLOWS.

1882. Badaloni, Giuseppe, M.D., Bologna, Italy.
1856. Baker, Albert, M.D., Batcombe Rectory, Bath.
1855. Beardsley, Amos, Bay Villa, Grange, Lancashire.
1850. Beneke, F. W., M.D., New York.
1850. Böhm, Professor, M.D., Vienna.
1850. Bottini, Enrico, M.D., Milan.
1855. Coates, Charles, M.D., F.R.C.P., 10, Circus, Bath, Consulting Physician to the Bath Royal United Hospital. c 3.
1850. Cox, William Isidore, Hawkesbury-Upton, Gloucestershire. c.
1853. Fallot, R., M.D., St. Laurent d'Aigonze, Montpelier, France.
1889. Frank, Philip, M.D., F.R.C.P., Cannes, France.
1876. Griffith, Richard Glyn, Allahabad, India.
1896. Hamilton, Allan McLane, M.D., 44, East Twenty-ninth Street, New York.
1864. Hasenfeld, Emmanuel, M.D., Pesth.
1875. Jones, Philip Sydney, M.D., F.R.C.S., Examiner in Medicine in the University of Sydney, Australia, Hon. Consulting Surgeon to the Sydney Infirmary.
1851. Kölliker, Albert, M.D., Professor of Anatomy and Physiology at the University of Wurzburg.
1876. Leighton, Walter H., M.D., Lowell, Massachusetts, U.S.A.
1851. Negri, Gaetano, M.D. Pisa.
1865. Peruzzi, Domenico, M.D., 22, Via Mazzini, Bologna.
1886. Rocha, A., M.D., Professor of Clinical Medicine at the University of Coimbra, Beira, Portugal.
1860. Roussel, M.D., Dean of the Faculty of Medicine, Montpelier.
Scharlan, Gus. W., M.D., Stettin, Prussia.
1876. Schmitz, Richard, M.D., Neuenahr.
1874. Schutgowsky, J., St. Petersburg.
1851. Stockwell, Thomas Goldesbrough, F.R.C.S., 6, Circus, Bath, Surgeon to the Bath Royal United Hospital.
1851. Williams, Charles, F.R.C.S. Edin., 48, Prince of Wales-road, Norwich; Senior Surgeon to the Norfolk and Norwich Hospital.

THE FELLOWS

OF

THE MEDICAL SOCIETY OF LONDON.

(Corrected to 1st November, 1900.)

Explanation of Abbreviations.

P.—President.	FM.—Fothergillian Gold Medallist.
VP.—Vice-President.	SM.—Silver Medallist.
T.—Treasurer.	O.—Orator.
L.—Librarian.	CFC.—Chairman, House and Finance Committee.
S.—Secretary.	§—Sec. for Foreign Correspondence.
C.—Councillor.	*—Life Members.
LL.—Lettsomian Lecturer.	Tr.—Trustee.

The number prefixed signifies the date of election. The figures appended indicate the number of Sessions served, and refer to past appointments ONLY.

1890. Abbot-Anderson, William Maurice, M.B., 37, Wimpole-street, W.
1888. Abbott, Charles Edward, M.R.C.S., 1, Wellington-place, Cheltenham.
1891. Abraham, Phineas S., M.D., 2, Henrietta-street, Cavendish-square, W.
1896. Abrahams, Bertram Lewis, M.B., 14, Welbeck-street, W.
1894. Achard, Alexander Louis, M.D., 9, Blandford-street, Portman-square, W.
1890. Ackland, Robert Craig, M.R.C.S., 54, Brook-street, Grosvenor-square, W.
1883. Acland, Theodore Dyke, M.D., 19, Bryanston-square, W. c 2.
1878. *Allchin, William Henry, M.D., F.R.S.E., 5, Chandos-street, Cavendish-square, W. sm, l 18, vp 2, o, c. *Councillor.*
1873. Allen, Henry Marcus, F.R.C.P. Edin., 17, Palmeira-square, Brighton.
1873. Allfrey, Charles Henry, M.D., Plas Newydd, St. Leonards-on-Sea.
1883. Allingham, Herbert W., F.R.C.S., 25, Grosvenor-street, W. c 3.
1872. *Allingham, William, F.R.C.S., 25, Grosvenor-street, W. c.
1894. Alston, William Evelyn, M.B., Wheathampstead, Herts.
1885. Anderson, John, M.D., C.I.E., 9, Harley-street, W. c 3.

1869. Armitage, Samuel Harris Tatham, M.D., 39, Grosvenor-street, W.
1894. Armstrong, William, M.R.C.S., J.P., Thorncliffe, Buxton.
1894. Ashe, William Percy, M.D., 17, Alexander-square, S.W.
1873. Atkinson, Edward, M.R.C.S., 93, Albion-street, Leeds.
1899. Auld, Alexander Gunn, M.D., 24, Welbeck-street, W.
1892. Ayres, Charles James, M.D., 55a, Welbeck-street, W.

1873. Bagshawe, Frederic, M.D., 35, Warrior-square, St. Leonards-on-Sea. c.
1892. Bailey, Henry Frederick, M.R.C.S., The Hollies, Lee-terrace, Lee, S.E. c 2.
1894. Bailey, Robert Cozens, M.S., 21, Welbeck-street, W.
1891. Baily, Percy J., M.B., County Asylum, Hanwell, W.
1898. Bain, William, M.D., Shaythorpe, York-place, Harrogate.
1876. *Baker, Henry Francis, F.R.C.S. Edin., 2, Mandeville-place, Manchester-square, W. c.
1890. Baker, William Henry, M.R.C.S., 152, Westbourne-grove, W. c 2.
1891. Ball, James Barry, M.D., 12, Upper Wimpole-street, W.
1881. Ballance, Charles Alfred, M.S., 106, Harley-street, W. s 2, c 3.
1884. Banks, Sir W. Mitchell, F.R.C.S., LL.D., 28, Rodney-street, Liverpool. o, c, ll. *Vice-President.*
1899. Barker, Arthur E. J., F.R.C.S., 87, Harley-street, W.
1898. Barnard, Harold Leslie, M.S., 9, Upper Wimpole-street, W.
1883. *Barnes, Robert, M.D., *Honorary Fellow* (*q. v.*).
1874. Barrett, Howard, M.R.C.S., 49, Gordon-square, W.C. c. *Vice-President.*
1896. Barrett, Walter Russell, M.R.C.S., 6, Chandos-street, Cavendish-square, W.
1884. Barrow, Albert Boyce, F.R.C.S., 37, Wimpole-street, W. c.
1900. Barter, William, M.D., 47, Greencroft-gardens, West Hampstead, N.W.
1886. Barwell, Richard, F.R.C.S., 55, Wimpole-street, W.
1884. Bateman, Frederick Augustus Newton, M.R.C.S., 4, Charles-street, St. James's-street, S.W.
1886. Batterham, John Williams, M.B., Bank House, Grand-parade, St. Leonards-on-Sea.
1888. Battle, William Henry, F.R.C.S., 49, Harley-street, W. c 2, s 2. *Councillor.*
1896. Bayliss, Richard Arthur, M.R.C.S., 5, Gay-street, Bath.
1882. Beach, Fletcher, M.B., Winchester House, Kingston-hill, Surrey, and 64, Welbeck-street, W. c.
1887. Beale, Edwin Clifford, M.B., 23, Upper Berkeley-street, W. c 2.
1891. Beale, Peyton T. B., F.R.C.S., 61, Grosvenor-street, W.
1880. Beevor, Charles Edward, M.D., 33, Harley-street, W. s 2, c. *Trustee.*

1889. Beevor, Sir Hugh Reeve, Bart., M.D., 17, Wimpole-street, W.
1900. Belfrage, S. Henning, M.B., 2, Montagu-place, Montagu-square, W.
1881. Bennet, Robert Ottiwell-Gifford, M.D., Tankerville House, Park-place, Buxton.
1883. Bennett, William Henry, F.R.C.S., 1, Chesterfield-street, Mayfair, W. c.
1900. Benson, Alfred, M.B., 79, Maida Vale, W.
1896. Berkeley, Comyns, M.B., 53, Wimpole-street, W.
1887. Berry, James, F.R.C.S., 21, Wimpole-street, W. *Councillor.*
1898. Best, William Harris, L.S.A., Ilford, Essex.
1873. Beveridge, James Spowart, M.R.C.P. Edin., Melton Constable, Norfolk.
1890. Bidwell, Leonard Arthur, F.R.C.S., 59, Wimpole-street, W.
1888. Bird, Matthew Mitchell, M.D., St. Mary's Hospital, W.
1850. *Birkett, John, F.R.C.S., 1, Sussex-gardens, W. vp, c 6.
1889. Bisshopp, Francis Robert Bryant, M.B., Belvedere, Lonsdale-gardens, Tunbridge Wells.
1886. *Black, William Gall, F.R.C.S., 2, George-square, Edinburgh.
1897. Blacker, George Francis, M.D., 11, Wimpole-street, W.
1881. Blaker, Walter Campbell, M.R.C.S., 4, Denmark-villas, Hove, Sussex.
1888. Blanc, Leon, M.D., Aix les Bains, France.
1892. Bland-Sutton, John, F.R.C.S., 48, Queen Anne-street, W.
1871. *Bloxam, John Astley, F.R.C.S., 75, Grosvenor-street, W. vp 2, s 2, c 3.
1867. Bond, Thomas, F.R.C.S., 7, The Sanctuary, Westminster, S.W. c.
1879. Bott, Henry, M.R.C.S., Brentford, Middlesex.
1886. Bourns, Newcome Whitelaw, M.D., 78, Redcliffe-gardens, S.W.
1886. Boustead, Robinson, M.D., Lieutenant-Colonel, R.A.M.C., c/o Messrs. King & Co., 45, Pall Mall, S.W.
1895. Bower, David, M.D., Springfield House, Bedford.
1889. Bowles, Robert Leamon, M.D., 16, Upper Brook-street, W. c 3.
1895. Boyd, Stanley, F.R.C.S., 134, Harley-street, W. c 2.
1899. Bradford, J. Rose, M.D., F.R.S., 8, Manchester-square, W.
1896. Bradley, James Edward Campbell, M.B., Greville House, Raynes Park, Wimbledon.
1889. Braine, C. Carter, F.R.C.S., 26, Wimpole-street, W.
1876. Brewer, Alexander Hampton, M.R.C.S., 136, Richmond-road, Dalston, N.E.
1896. Bridges, E. Chittenden, M.B., Priory-mansions, Drayton-gardens, South Kensington, S.W.
1873. Bridgwater, Thomas, M.B., LL.D., J.P., Harrow, Middlesex.
1893. Broadbent, John Francis Harpin, M.B., 35, Seymour-street, W.
1862. Broadbent, Sir William Henry, Bart., M.D., F.R.S., 84, Brook-street, W. p, vp, o, ll, c 4.
1900. Broadbent, Walter, M.D., 50, Brunswick-square, Brighton.

1890. Brook, William Frederick, F.R.C.S., Longlands House, Swansea.
1898. Brooke, Gilbert E., L.R.C.P., East Harbour, Turk's Island, West Indies.
1878. Brown, Andrew, M.D., 27, Lancaster-road, Belsize-park, N.W.
1889. Browne, George Buckston, M.R.C.S., 80, Wimpole-street, W. c 2.
1873. Browne, Lennox, F.R.C.S. Edin., 15, Mansfield-street, Portland-place, W.
1887. Bruce, John Mitchell, M.D., 23, Harley-street, W. vp 2, c 2. *Lettsomian Lecturer. Councillor.*
1873. Brunjes, Martin, M.R.C.S., 270, Willesden-lane, N.W.
1898. Brunton, John, M.B., 16, Endsleigh-street, Tavistock-square, W.C.
1874. *Brunton, Sir T. Lauder, M.D., F.R.S., 10, Stratford-place, W. ll, vp, c 4, sm.
1850. *Bryant, Thomas, F.R.C.S., 65, Grosvenor-street, W. p, vp, ll, s 2, c 4. *Trustee.*
1897. Bunch, John Le Mare, M.D., 32, Wimpole-street, W.
1885. *Bunny, J. Brice, M.R.C.S., Warre House, Bishops Lydeard, Taunton.
1886. Butler-Smythe, Albert Charles, F.R.C.S., 76, Brook-street, W.
1872. Byas, Edward Hegley, M.R.C.S., 10, Cambridge-gate, Regent's Park, N.W.

1886. Cahill, John, M.D., 12, Seville-street, Lowndes-square, Hyde Park, S.W. c 3.
1892. Caldwell, Robert, F.R.C.S., Major, R.A.M.C., care of Messrs. Holt and Co., 17, Whitehall-place, S.W.
1893. Caley, Henry Albert, M.D., 24, Upper Berkeley-street, W. c. *Councillor.*
1891. Calvert, James, M.D., St. Bartholomew's Hospital, E.C. s 2. *Councillor.*
1897. Cameron, Ewan Gordon, M.R.C.S., 64, Great Portland-street, W.
1892. Cargill, Lionel Vernon, F.R.C.S., 15, Stratford-place, W.
1892. Carless, Albert, F.R.C.S., 10, Welbeck-street, W.
1882. Carpenter, Arthur Bristowe, M.B., Wykeham House, Bedford Park, Croydon.
1889. Carr, John Walter, M.D., 19, Cavendish-place, W.
1897. Carré, L. J. Gerard, M.D., 27th Baluchis, Uganda, British East Africa.
1871. Carter, Robert Brudenell, F.R.C.S., 31, Harley-street, W. p, vp, o, ll, c 4.
1889. Cartwright, Alexander, M.R.C.S., 32, Old Burlington-street, W.
1876. Cartwright, S. Hamilton, *Travelling.*
1899. Cathcart, George Clark, M.B., 35, Harley-street, W.
1876. *Cathcart, Samuel, M.R.C.P. Edin., 11, Hornsey-lane-gardens, Highgate, N.
1889. Cautley, Edmund, M.D., 15, Upper Brook-street, W.
1896. Chapman, Charles William, M.D., 21, Weymouth-street, W.

1885. Chasseaud, William, M.D., Smyrna, Asia Minor.
1889. Cheyne, Watson, F.R.C.S., F.R.S., 75, Harley-street, W. ll, c.
1877. *Chisholm, Edwin, M.D., 82, Darlinghurst-road, Sydney, New South Wales.
1895. Christmas, Robert William Samuel, M.R.C.S., 23, Hopton-road, Streatham, S.W.
1871. Churton, Thomas, M.D., 35, Park-square, Leeds. c.
1875. Clark, Andrew, F.R.C.S., 71, Harley-street, W. c.
1883. Clarke, William Bruce, F.R.C.S., 51, Harley-street, W. c.
1894. Clinch, Thomas Aldous, M.D., County Asylum, Winterton, Ferryhill, co. Durham.
1879. *Clutton, Henry Hugh, F.R.C.S., 2, Portland-place, W. vp 2, c 3.
1899. Cobb, Ernest Herbert, M.R.C.S., Victoria Hospital, Folkestone.
1894. Cochrane, James Mackeand, M.D., 10, Weymouth-street, W.
1849. *Cockle, John, M.D., The Lodge, West Molesey, Surrey. p, vp, o, l 3, c 3, sm.
1899. Cohen, George Alexander, M.B., 17, Grand-parade, Harringay, N.
1893. Cole, Robert Henry, M.D., Moorcroft, Hillingdon, Uxbridge.
1887. Collier, William, M.D., St. Mary's Entry, Oxford. c.
1892. Colman, Walter S., M.D., 22, Wimpole-street, W. c.
1871. Cook, John, M.D., 1, Nottingham-terrace, Regent's Park, N.W.
1862. Cooper, Alfred, F.R.C.S., 9, Henrietta-street, Cavendish-square, W. c 3, vp.
1888. Cooper, Arthur, M.R.C.S., 20, Old Burlington-street, W.
1894. Cooper, Peter, M.R.C.S., 35, Shooters-hill-road, Blackheath, S.E.
1894. Corbould, Victor A. L. E., M.D., 43, Victoria-road, Kensington, W.
1872 Corfield, William Henry, M.D., 19, Savile-row, W. c.
1899. Corner, Harry, M.D., Brook House, Southgate, N.
1898. Cotton, Holland John, M.D., 2, Eccleston-houses, St. George's-road, S.W.
1891. Coumbe, John Batten, F.R.C.S., 55, High-street, Lowestoft.
1879. Coupland, Sidney, M.D., 16, Queen Anne-street, W. vp 2, c 3.
1889. Courtney, Guy Budd, M.B., 28, Augusta-road, Ramsgate.
1873. Craven, Sir Robert Martin, F.R.C.S., J.P., 14, Albion-street, Hull.
1889. Crawford, James, M.D., Grosvenor-mansions, 80, Victoria-street, S.W.
1899. Creasy, L. Eliot, M.R.C.S., 35a, Welbeck-street, W.
1871. Crichton-Browne, Sir James, M.D., F.R.S., 61, Carlisle-place-mansions, S.W. p, o, c 2.
1881. Cripps, William Harrison, F.R.C.S., 2, Stratford-place, W. c 2.
1880. Critchett, George Anderson, F.R.C.S. Edin., 21, Harley-street, W.
1880. Crocker, Henry Radcliffe, M.D., 121, Harley-street. c 3.
1881. Cross, Francis Richardson, F.R.C.S., Worcester House, Clifton, Bristol. *Orator.*
1890. Cullingworth, Charles James, M.D., 14, Manchester-square, W. c 3.
1874. Cumberbatch, Alphonso Elkin, F.R.C.S., 80, Portland-place, W. c 2.

1897. Dakin, William Radford, M.D., 18, Grosvenor-street, W.
1894. Dalby, John Lyttleton, M.R.C.S., 13, Buckingham-road, Brighton.
1871. Dalby, Sir William Bartlett, F.R.C.S., 18, Savile-row, W. P, C 2.
1881. Dallaway, Dennis Joseph William, L.R.C.P. Edin., Piccadilly Club, W. C. *Councillor.*
1890. Davis, Henry, M.R.C.S., 60, Queen Anne-street, W.
1889. *Davison, James, M.D., "Streate Place," Bath-road, Bournemouth.
1897. Davison, Rashell, M.D., Vernham, New Malden, Surrey.
1880. Davson, Smith Houston, M.D., Campden Villa, 203, Maida-vale, W. C 3.
1868. *Davy, Richard, F.R.C.S., F.R.S.E., Burstone House, Bow, North Devon. VP, S 2, SM, § 2.
1876. Dawes, Richard St. Mark, M.R.C.S., Gawler, South Australia.
1897. Dawson, Bertrand, M.D., 110, Harley-street, W.
1880. Dawson, Yelverton, M.D., Heathland, Southbourne-on-Sea, Christchurch, Hants.
1894. Dean, Henry Percy, M.S., 69, Harley-street, W.
1883. Dent, Clinton Thomas, F.R.C.S., 61, Brook-street, Grosvenor-square, W. C 3.
1897. Des Vœux, Harold A., M.D., 14, Buckingham-gate, S.W.
1891. Diver, Ebenezer, M.D., "Garstone," St. Peter's-road, St. Margaret's, Twickenham.
1893. Dockrell, Morgan, M.D., 9, Cavendish-square, W.
1885. Dodd, Henry Work, F.R.C.S., 136, Harley-street, W.
1882. Dolan, Thomas Michael, M.D., Horton House, Halifax. FM 1882.
1881. Doran, Alban Henry Griffiths, F.R.C.S., 9, Granville-place, W. C 4, O, § 3.
1898. Douglas, John L., M.D., 42, Central-hill, Norwood, S.E.
1890. Douglas, William, M.D., 4, St. Alban's-place, Regent-street, S.W.
1871. Dowse, Thomas Stretch, M.D., 14, Welbeck-street, W. § 3, C 3.
1897. *Drage, Lovell, M.D., Hatfield, Herts.
1886. Duckworth, Sir Dyce, M.D., 11, Grafton-street, Piccadilly, W. C.
1884. Duncan, William, M.D., 6, Harley-street, W.
1884. Durham, Frederick, F.R.C.S., 52, Brook-street, W.

1895. Ealand, Hugh Fawsit, L.R.C.P. Edin., The Moat, Puckeridge, Herts.
1896. Earle, Walter George, M.R.C.S., 44, Upper Grosvenor-road, Tunbridge Wells.
1899. Eastes, George Leslie, M.B., 35, Gloucester-terrace, W.
1891. Eastes, Thomas, M.D., 3, Shakespeare-terrace, Folkestone.
1895. Eccles, William McAdam, M.S., 124, Harley-street, W.
1892. Eddowes, Alfred, M.D., 28, Wimpole-street, W.
1860. *Edmunds, James, M.D., 26, Manchester-square, W.
1880. Edwards, Frederick Swinford, F.R.C.S., 55, Harley-street, W. C 3.

1897. Elgood, Charles Reginald, M.D., 8, Clarence-villas, Windsor.
1899. Elliot, Norman Bruce, F.R.C.S., 27, Warwick-square, S.W.
1868. Elliott, George Frederick, M.D., 1, Albion-street, Hull.
1882. Elliott, Thomas, M.D., Monson-place, Tunbridge Wells.
1883. English, Edgar, M.R.C.S., St. Mary's-road, Doncaster.
1880. English, Thomas Johnston, M.D., 128, Fulham-road, S.W.
1889. Esler, Robert, M.D., 4, Queen's-road, Peckham, S.E.
1899. Evans, J. Morgan, M.R.C.S., J.P., Llandrindod Wells, Radnorshire.
1894. Evans, Willmott Henderson, F.R.C.S., 13, Taviton-street, Gordon-square, W.C.
1898. Eve, Frederic S., F.R.C.S., 125, Harley-street, W.
1883. Ewart, Sir Joseph, M.D., J.P., Dep. Surgeon-General, Bengal Army (*retired*), Bewcastle, Dyke-road, Brighton. c.
1877. Ewart, William, M.D., 33, Curzon-street, Mayfair, W. c 3. *Hon. Sec. Foreign Correspondence.*

1884. Fardon, Edward Ashby, M.R.C.S., Middlesex Hospital, W.
1873. *Fayrer, Sir Joseph, Bart., K.C.S.I., LL.D., M.D., F.R.S., *Honorary Fellow* (*q. v.*).
1896. Fayrer, Joseph, M.D., Captain, R.A.M.C., India.
1898. Felkin, Robert William, M.D., 23, Henrietta-street, Cavendish-square, W.
1884. Fenton, Frederick Enos, F.R.C.S., 33, Welbeck-street, W.
1888. Fenwick, Bedford, M.D., 20, Upper Wimpole-street, W.
1885. Fenwick, Edwin Hurry, F.R.C.S., 14, Savile-row, W. c.
1887. Ferrier, David, M.D. Edin., F.R.S., 34, Cavendish-square, W. c.
1878. Field, George, M.R.C.S., 34, Wimpole-street, W. c.
1894. Fisher, Frederick Charles, F.R.C.S., King's Langley, Herts.
1876. Fisher, Frederic Richard, F.R.C.S., 18, Wimpole-street, W.
1899. Fletcher, H. Morley, M.D., 98, Harley-street, W.
1884. Flint, Arthur, M.D., J.P., Westgate Lodge, Westgate-on-Sea.
1878. *Fonmartin, Henry de, M.D., 26, Newberry-terrace, Lower Bullar-street, Southampton.
1893. Forbes, Norman Hay, F.R.C.S. Edin., Drumminor, Tunbridge Wells.
1899. Ford, Frank Chubb, M.B., 47, Ladbroke-square, W.
1879. Fowler, James Kingston, M.D., 35, Clarges-street, Mayfair, W. s 2, c 2, vp 2, o.
1873. Fox, Arthur Edward Wellington, M.B., C.M., Ennox Lodge, Hinton, Charterhouse, near Bath. c.
1887. Fox, Fortescue, M.D., Strathpeffer Spa, Ross-shire.
1871. Fox, Francis, M.R.C.S., 15, Arundel-gardens, Kensington Park, W. c 3.
1885. *Fox, R. Hingston, M.D., 23, Finsbury-square, E.C.
1879. Fox, Thomas Colcott, M.B., 14, Harley-street, W. s 2, c 2.
1868. Freer, Alfred, M.R.C.S., J.P., Stourbridge, Worcestershire.
1896. Freyer, P. Johnston, M.Ch., Surgeon-Lieut.-Colonel (*retired*), 46, Harley-street, W.

1886. Frith, Baptist Gamble, M.B., Drumgay, Grove-road, Guildford.
1884. Frost, William Adams, F.R.C.S., 30, Cavendish-square, W. c 2.

1895. Galloway, James, M.D., 54, Harley-street, W.
1862. *Gant, Frederick James, F.R.C.S., *Honorary Fellow* (*q. v.*).
1899. Gardener, William Frederick, M.R.C.S., Darley House, Venner-road, Sydenham, S.E.
1847. *Garrod, Sir Alfred Baring, M.D., F.R.S., 10, Harley-street, W. p, vp 2, ll, o, c 9.
1893. Garson, John George, M.D., 122, Harley-street, W.
1891. Gaster, Aughel, M.D., 68, Greencroft-gardens, South Hampstead, N.W.
1887. Gay, John, M.R.C.S., 119, Upper Richmond-road, Putney, S.W.
1856. *Gibbon, Septimus, M.B., 39, Oxford-terrace, W.
1882. Gibbons, Robert Alexander, M.D., 29, Cadogan-place, S.W.
1893. Gibson, Charles, M.D., Vanderbilt-court, Harrogate.
1897. Gibson, George Alexander, M.D., 17, Alva-street, Edinburgh.
1881. Giffard, Douglas W., M.R.C.S., 5, Pavilion-parade, Brighton.
1867. Gill, William, M.R.C.S., 11, Russell-square, W.C. c.
1894. Gill, Richard, F.R.C.S., 72, Wimpole-street, W.
1900. Godfrey, Thomas Henry, M.B., The Red House, Church-end, Finchley, N.
1869. *Godson, Clement, M.D., 84, Brook-street, W. vp, c 3, s 2, sm.
1873. Goodsall, David Henry, F.R.C.S., 17, Devonshire-place, Portland-place, W. vp 2. c, s 2, sm, cfc 7. *Treasurer; Chairman, House and Finance Committee.*
1878. *Gould, Alfred Pearce, M.S., 10, Queen Anne-street, W. vp 2, s 2, c 4, o.
1896. Gow, William John, M.D., 27, Weymouth-street, W.
1876. Gowers, Sir William Richard, M.D., F.R.S., 50, Queen Anne-street, W. vp, c, sm, ll. fm. 1893.
1887. Grant, James Edward Roney, M.R.C.S., 2, Charing Cross-chambers, Duke-street, Adelphi, W.C.
1896. Grant, J. Dundas, M.D., 8, Upper Wimpole-street, W.
1881. Green, Thomas Henry, M.D., 74, Wimpole-street, W. c 2. vp.
1886. Greves, Edwin Hyla, M.D., Rodney House, Bournemouth.
1873. Grieve, the Hon. Robert, M.D., C.M.G., British Guiana.
1885. Griffiths, Charles Thomas, L.R.C.P., 125, Soho-hill, Birmingham.
1884. Griffiths, Herbert Tyrrell, M.B., 5, Kensington-square, W.
1880. Gristock, William, M.D. Lond., 101, London-road, W. Croydon.
1891. Guthrie, Leonard G., M.D., 15, Upper Berkeley-street, W.

1886. Habershon, S. Herbert, M.D., 88, Harley-street, W.
1891. Hadley, Wilfred J., M.D., 58, Harley-street, W.
1887. Haig, Alexander, M.D., 7, Brook-street, W.
1881. Hall, Charles Ross, M.R.C.S., Hatfield, Herts.

1874. *Hall, Francis de Havilland, M.D., 47, Wimpole-street, W. vp 2, ll, c 7, s 2, sm.
1891. Handfield-Jones, Montagu, M.D., 35, Cavendish-square, W.
1891. Harold, John Patrick, M.R.C.S., 91, Harley-street, W.
1895. Harrison, Charles James, M.D., Rossetti-mansions, Chelsea, S.W.
1871. Harrison, Reginald, F.R.C.S., 6, Lower Berkeley-street, Portman-square. p, vp 2, ll, c 2.
1883. *Hartridge, Gustavus, F.R.C.S., 12, Wimpole-street, W.
1882. Harvey, John Stephenson Selwyn, M.D., 1, Astwood-road, Cromwell-road, S.W.
1882. Haslam, William Frederick, F.R.C.S., 24, York-road, Edgbaston, Birmingham. c 3.
1852. *Haward, Edwin, M.D., 34a, Gloucester-place, W.
1898. Haydon, Arthur George, M.D., 74, Cotleigh-road, West Hampstead, N.W.
1897. Hayman, Albert Stephen, L.R.C.P., 26, Welbeck-street, W.
1899. Hays, Alexander Glencorse, L.R.C.P., Eydon, Byfield, Northamptonshire.
1890. Hebb, Frederick Theodore, M.R.C.S., 98, Oakley-street, Chelsea-embankment, S.W.
1897. Hedley, William Snowdon, M.D., 8, Mansfield-street, W.
1891. Hensman, Frank, M.R.C.S., Lieut.-Colonel, R.A.M.C., Underhill, Sandgate, Kent.
1883. Herman, George Ernest, M.B., 20, Harley-street, W. *Councillor.*
1879. Heron, George Allan, M.D., 57, Harley-street, Cavendish-square, W. c 3.
1883. Herschell, George A., M.D., 76, Wimpole-street, W.
1883. Hewitt, Frederick William, M.D., 14, Queen Anne-street, W. c.
1892. Hill, William, M.D., 26, Weymouth-street, W.
1873. Hobson, William Henry, M.R.C.S., 38, Leinster-gardens, Hyde Park, W.
1895. Hodgson, George G. Graham, M.R.C.S., The Cedars, Chertsey, Surrey.
1879. Hogg, Arthur John, M.R.C.S., Leslie Lodge, Haven-green, Ealing, W.
1884. Holland, Charles Edward, M.B., "Airdrie," The Avenue, Kew Gardens, Surrey.
1868. Holman, Constantine, M.D., 26, Gloucester-place, Portman-square, W. vp 2, c 5.
1897. Holroyde, John, M.R.C.S., Camden House, Chatham.
1881. Hood, Donald William Charles, M.D., 43, Green-street, W. c 2.
1896. Horsley, Victor, F.R.C.S., F.R.S., 25, Cavendish-square, W. fm 1896.
1883. *Hovell, T. Mark, F.R.C.S. Edin., 105, Harley-street, W.
1899. Hubbard, Arthur John, M.D., 69, Barrowgate-road, Chiswick, W.
1890. Hughes, Edgar, F.R.C.S., 91, Onslow-gardens, South Kensington, S.W.
1864. Hume, Frederick Henry, M.D., 53, Devonshire-street, Islington, N.

1884. Hunter, Sir Guyer, M.D., K.C.M.G., East India U.S. Club, St. James's, S.W.
1889. Hunter, William, M.D., 103, Harley-street, W.
1881. Hutchinson, Jonathan, F.R.C.S., F.R.S., 15, Cavendish-square. P, LL, C 5, O.
1892. Hutchinson, Jonathan, Jun., F.R.C.S., 1, Park-crescent, Portland-place, W.
1875. Hutchinson, Samuel John, M.R.C.S., 64, Brook-street, W.
1897. Hutchison, Robert, M.D., Toynbee Hall, Commercial-road, E.

1889. I'Anson, William Andrew, M.R.C.S., Denton Hall, Newcastle-on-Tyne.
1891. Isaac, G. Washington, M.B., 75, Gower-street, W.C. C. *Councillor.*

1884. *Jackson, Frederick William, M.D., Yorkgate House, Broadstairs.
1868. Jackson, John Hughlings, M.D., F.R.S., 3, Manchester-square, W. P, VP, O, C 5.
1899. Jacobs, Henry Ralph, M.R.C.S., 5, Hollis-road, Northampton.
1898. Jaffrey, Francis, F.R.C.S., 33, Nottingham-place, W.
1884. *Jennings, Charles Egerton, M.D., 48, Seymour-street, W.
1886. Jervis, Arthur, M.D., 52, York-street, Portman-square, W.
1883. Jessop, Walter Hamilton, F.R.C.S., 73, Harley-street, W.
1893. Johnston, George Francis, M.D., 3, Montagu-place, Montagu-square, W.
1886. Johnston, James, M.D., 53, Prince's-square, Bayswater, W.
1893. Joll, Boyd Burnett, M.B., 27, Bedford-square, W.C.
1888. Jones, Arthur Henry, M.D., 45, Sheep-street, Northampton.
1899. Jones, E. J. Trevor, M.D., Ty-mawr, Aberdare, S. Wales.
1893. Jones, Henry Lewis, M.D., 61, Wimpole-street, W.
1890. Jones, H. Macnaughton, M.D., 131, Harley-street, Cavendish-square, W.
1888. Jones, John Talfourd, M.B., Rosebank, South-terrace, Eastbourne, Sussex.
1892. *Jones, Robert, F.R.C.S., 11, Nelson-street, Liverpool.
1896. Jones, Robert, M.D., Claybury Asylum, Woodford Bridge, Essex.
1893. Joule, John Samuel, M.D., 32, Maida-hill West, W.
1877. Juler, Henry Edward, F.R.C.S., 23, Cavendish-square, W. C 2.

1889. Kauffmann, Otto Jackson, M.D., 22, Broad-street, Birmingham.
1891. Keegan, Denis Francis, M.D., Brigade Surgeon-Lieutenant-Colonel, East India United Service Club, St. James's-square, S.W.
1896. Keep, A. Corrie, M.C., 14, Gloucester-place, Portman-square, W.
1884. Keetley, Charles Bell, F.R.C.S., 56, Grosvenor-street, W. C 3.
1899. Kellock, Thomas Herbert, F.R.C.S., 8, Queen Anne-street, W.
1847. *Kellock, William Berry, M.D., 94, Stamford-hill, N.

1891. Kelson, William Henry, M.D., 16, Old Burlington-street, W.
1890. Ker, Hugh Richard, F.R.C.S. Edin., Tintern, 2, Balham-hill, S.W.
1886. Kidd, Percy, M.D., 60, Brook-street, W. c 2.
1895. King, Arthur, M.B., Belmont, Station-road, Watford, Herts.
1897. Kingscote, Ernest, M.B., 31, Lower Seymour-street, W.
1883. Knapton, George, L.R.C.P. Edin., Craven House, Moss-lane, Manchester.

1898. Lack, H. Lambert, M.D., 48, Harley-street, W.
1889. Lake, Richard, F.R.C.S., 19, Harley-street, W.
1895. Lane, James Ernest, F.R.C.S., 46, Queen Anne-street, W.
1881. Langton, John, F.R.C.S., 62, Harley-street, W. c 2.
1890. Law, Edward, M.D. Edin., 8, Wimpole-street, W.
1858. *Lawson, George, F.R.C.S., 12, Harley-street, W. vp 2, c 3.
1893. Lee, E. Samuel, M.D., 39, Hogarth-road, South Kensington, S.W.
1887. Leggatt, Charles Ashley Scott, M.D., 2, Walton-place, S.W.
1894. Lendon, Edwin Harding, M.B., 162, Holland Park-avenue, W.
1886. Lewers, Arthur Hamilton Nicholson, M.D., 72, Harley-street, W.
1895. Lewis, Ernest Wool, M.R.C.S., 12, King-street, Hammersmith, W.
1867. Lichtenberg, George, M.D., 47, Finsbury-square, E.C. c 2.
1898. Ling, Maurice Edward, M.R.C.S., 5, West Halkin-street, S.W.
1878. Lister, the Right Hon. Lord, D.C.L., LL.D., F.R.C.S., President of the Royal Society, 12, Park-crescent, Portland-place, W. o.
1896. Lister, Thomas D., F.R.C.S., 95, Wimpole-street, W.
1889. Little, Ernest Muirhead, F.R.C.S., 40, Seymour-street, Portman-square, W.
1889. Little, Fletcher, M.D., 32, Harley-street, W.
1878. Lockwood, Charles Barrett, F.R.C.S., 19, Upper Berkeley-street, W. vp 2, c 2, s 2.
1873. Loe, James Scarborough, M.R.C.S., Grange-avenue, Chapeltown-road, Leeds.
1881. Lorimer, G., M.D., 9, Terrace-road, Buxton, Derbyshire.
1899. Low, Richard Marsden Pilkington, M.B., 70, Philbeach-gardens, S.W.
1899. Low, Vincent Warren, F.R.C.S., 27, Queen Anne-street, W.
1868. Lowe, John, M.D., J.P., Oatlands Wood, Weybridge. c 3.
1897. Luff, Arthur Pearson, M.D., 31, Weymouth-street, W.
1889. Lunn, Henry Simpson, M.D., 5, Endsleigh-gardens, N.W.
1889. Lush, Percy, M.B., 4, Maresfield-gardens, Hampstead, N.W.
1899. Lynes, John, M.R.C.S., 2, Argyll-road, Kensington, W.

1884. MacBryan, Henry Crawford, L.R.C.P. Edin., Kingsdown House, Box, Wilts.
1894. McCann, Frederick John, M.B., 5, Curzon-street, Mayfair, W.
1885. McConnel, Henry Wilson, M.B., Great Ryburgh, Fakenham, Norfolk.

1871. MacCormac, Sir William, Bart., K.C.V.O., 13, Harley-street, W., President of the Royal College of Surgeons. P, VP, § 2 C 4 O.

1885. MacGeagh, Thomas Edwin Foster, M.D., 23, New Cavendish-street, W.

1896. Macgregor, Patrick Fraser, M.D., "Ashley," Cambridge Park, Wanstead, Essex.

1873. McHardy, Malcolm Macdonald, F.R.C.S. Edin., 5, Savile-row, W.

1894. Mackenzie, Hector William Gavin, M.D., 59, Welbeck-street, W. C.

1880. Mackenzie, Stephen, M.D., 18, Cavendish-square, W. VP 2, C 4, LL.

1881. Maclagan, Thomas John, M.D., 9, Cadogan-place, S.W. C 3.

1861. MacLaren, Alexander Connell, 60, Harley-street, W.

1891. Maclean, Allan, L.S.A., 24, Harwood-road, Fulham, S.W.

1887. Macready, Jonathan Foster Christian Horace, F.R.C.S., 42, Devonshire-street, W. C 2.

1883. Maddick, Edmund Distin, F.R.C.S. Edin., 47, Portland-place, W.

1885. Maguire, Robert, M.D., 4, Seymour-street, W. C 4, S 2. *Councillor.*

1893. Mahomed, Arthur George Sulieman, M.R.C.S., Astolat, Poole-road, Bournemouth.

1890. Malcolm, John David, F.R.C.S. Edin., 13, Portman-street, Portman-square, W.

1893. Malcolm, William A., M.B., Oak House, 421, Holloway-road, N.

1887. Mantle, Alfred, M.D., Savile-place, Halifax.

1883. Mapother, Edward Dillon, M.D., 32, Cavendish-square, W. C 3.

1898. Marriott, Charles William, M.D., Aubrey House, Bath-road Reading.

1891. Marsh, Howard, F.R.C.S., 30, Bruton-street, W.

1869. Marshall, William, M.D., Torrieburn, Barnes, S.W. C.

1889. Martin, John Michael Harding, M.D., Arnheim, Blackburn, Lancashire.

1890. Martin, Sidney, M.D., F.R.S., 10, Mansfield-street, W. C. *Hon. Librarian.*

1893. Mason, George Armstrong, M.B., 45, George-street, Portman-square, W.

1884. Matheson, Farquhar, M.B., 11, Soho-square, W.

1891. Maude, Arthur, M.R.C.S., Winterton House, Westerham, Kent.

1892. May, Chichester Gould, M.D., 59, Cadogan-place, S.W.

1891. May, William Page, M.D., 49, Welbeck-street, W.

1884. Meredith, William Appleton, F.R.C.S., 21, Manchester-square, W. C.

1897. Miles, William Ernest, F.R.C.S., 17, Devonshire-place, Portland-place, W.

1895. Monier-Williams, Montagu Sneade Faithfull, M.R.C.S., 54, Onslow-gardens, S.W.

1883. Morgan, John Hammond, F.R.C.S., 68, Grosvenor-street, W. VP 2, LL, S 2, C 2. *President.*

1899. Morier, Charles G. D., L.R.C.P.I., 1, Hamilton-terrace, N.W.

1893. Morison, Alexander, M.D., 14, Upper Berkeley-street, W.
1871. Morley, Alexander, 42, Albemarle-street, W. *Councillor.*
1895. Morley, Frank, M.R.C.S., 42, Albemarle-street, W.
1881. Morris, Henry, F.R.C.S., 8, Cavendish-square, W. c.
1878. Morris, Malcolm Alexander, F.R.C.S. Edin., 8, Harley-street, W. c 2.
1882. Morton, Andrew Stanford, F.R.C.S., 133, Harley-street, W.
1884. *Moullin, Charles William Mansell, F.R.C.S., 69, Wimpole-street, W. *Councillor.*
1899. Mullick, Sarat Kumar, M.B., Hospital for Diseases of the Heart, Soho-square, W.
1894. Murray, Charles Stormont, L.R.C.S. Edin., 85, Gloucester-place, W.
1884. Murray, Fred., M.B., Durbanville, Cape Colony, South Africa.
1890. Murray, George, M.R.C.S., 34, Wimpole-street, Cavendish-square, W.
1886. Murray, Hubert Montague, M.D., 25, Manchester-square, W. c 3.
1879. Murrell, William, M.D., 17, Welbeck-street, W.

1894. Naumann, J. C. Francis, M.D., 125, Gower-street, W.C.
1889. *Nias, J. Baldwin, M.D., 5, Rosary-gardens, South Kensington, S.W. c 2. *Councillor.*
1880. Nix, Edward James, M.D., 11, Weymouth-street, W. c 3.

1887. Oakley, Adam Robert Hamilton, L.R.C.P., Treath, Hornchurch, Essex.
1898. O'Brien, Christopher, M.B., 135, North Strand-road, Dublin.
1894. O'Callaghan, Robert, F.R.C.S.I., 137, Harley-street, W.
1885. Ogilvie, Leslie, M.B., 46, Welbeck-street, W.
1884. Ogle, Charles John, M.R.C.S., 1, Cavendish-place, W.
1884. Oliver, George, M.D., West End Park, Harrogate.
1892. Openshaw, Thomas Horrocks, F.R.C.S., 16, Wimpole-street, W.
1899. Ord, George William, M.R.C.S., 58, Queen's-road, Richmond, Surrey.
1875. Ord, William Miller, M.D., The Hall, Salisbury. p, o, c 4.
1892. Ord, William Wallis, M.D., The Hall, Salisbury.
1887. Ormerod, Joseph Arderne, M.D., 25, Upper Wimpole-street, W. c.
1889. Orton, George Hunt, M.B., 1a, Campden Hill-road, Kensington, W.
1894. Osborn, Samuel, F.R.C.S., J.P., 1a, Devonshire-street, Portland-place, W.
1880. Oswald, James Waddell Jeffreys, M.D., 245, Kennington-road, S.E. c 3.
1883. Owen, Charles J. Rayley, M.R.C.S., 14, Devonshire-terrace, Hyde Park, W. c.
1878. *Owen, Edmund, F.R.C.S., 64, Great Cumberland-place, W. p, o, vp 2, c 3, s 2, sm, ll. *Trustee.*
1881. Owen, Isambard, M.D., 40, Curzon-street, Mayfair, W. s 2, c 4. *Vice-President.*

1880. Palmer, Frederick Stephen, M.D., Compton Lodge, East Sheen, S.W.
1882. Palmer, William Pitt, M.B., Kirkham, Babbacombe, Torquay.
1877. *Paramore, Richard, M.D., 2, Gordon-square, W.C.
1881. Parrott, Edward John, M.R.C.S., The Thorn, Hayes, Middlesex.
1871. Parsons, Francis Henry, M.D., "The Hurst," West Worthing.
1885. Pasteur, William, M.D., 4, Chandos-street, Cavendish-square, W. s 2, c. 2. *Trustee.*
1872. Patten, Charles Arthur, M.R.C.S., Marpool House, Ealing, W.
1891. Patterson, Charles Sumner, M.B., 40, Highbury-place, N.
1890. Pattison, Edward Seton, M.R.C.S., Granville House, Fulham-road, S.W.
1854. *Pavy, Frederick William, M.D., F.R.S., 35, Grosvenor-street, W. vp, ll, c.
1881. *Peacey, William, M.D., Rydal Mount, St. John's-road, Eastbourne.
1883. Peck, Edward George, M.A., The Willows, Queensbury, Bradford, Yorks.
1871. Pedler, George Henry, M.R.C.S., 6, Trevor-terrace, Knightsbridge, S.W.
1883. Perigal, Arthur, M.D., New Barnet, Herts.
1878. Philipps, Sutherland Rees, M.D., 2, Berkeley-place, Cheltenham.
1894. Phillipps, William Alfred, M.D., 13, John-street, Berkeley-square, W.
1876. Phillips, Charles Douglas Ferguson, M.D., F.R.S.E., 10, Henrietta-street, Cavendish-square, W. c 3.
1873. Phillips, George Richard Turner, M.R.C.S., 24, Palace-court, Bayswater-hill, W. c 2.
1883. Phillips, Sidney Philip, M.D., 62, Upper Berkeley-street, Portman-square, W.
1883. Pick, Thomas Pickering, F.R.C.S., 18, Portman-street, W. c 2.
1883. Pitts, Bernard, F.R.C.S., 109, Harley-street, Cavendish-square. c 5, s 2.
1899. Poore, George Vivian, M.D., 32, Wimpole-street, W.
1890. Pope, Harry Campbell, M.D., 280, Goldhawk-road, Shepherd's Bush, W. *Councillor.*
1871. Powell, Sir Richard Douglas, Bart., M.D., 62, Wimpole-street, W. p, vp, c 5, o.
1891. Powell, William Wyndham, F.R.C.S., 16, Old Burlington-street W.
1891. Preston, Theodore Julian, M.R.C.S., Fleet Surgeon, Royal Navy, 238, Portsdown-road, Maida Vale, W.
1891. *Prickett, Marmaduke, M.D., 27, Oxford-square, W.
1885. Pringle, John James, M.B., 23, Lower Seymour-street, W.
1889. Pritchard, Owen, M.D., 41, Gloucester-square, W.
1898. Probyn-Williams, Robert James, M.D., 13, Welbeck-street, W.

1892. Ramsay, James, M.D., High Peter-gate, York.
1894. Rankin, Guthrie, M.D., 4, Chesham-street, Belgravia, S.W.

1881. Ranking, John Ebenezer, M.D., Hanover House, Tunbridge Wells.
1894. Rayner, Herbert Edward, F.R.C.S., Harcourt House, Camberley, Surrey.
1859. *Rayner, John, M.D., Swaledale House, Highbury-quadrant, N.
1890. Reid, John, M.B., Albert-road, Buckhurst Hill, Essex.
1882. Reid, Thomas Whitehead, M.D., 34, St. George's-place, Canterbury.
1872. *Richards, Joseph Peeke, M.R.C.S., 6, Freeland-road, Ealing, W. c 3.
1899. Richardson, Sidney William F., F.R.C.S., St. Thomas's Hospital, S.E.
1868. *Roberts, David Lloyd, M.D., F.R.S.E., 11, St. John's-street, Manchester.
1857. *Roberts, David Watkin, M.D., 56, Manchester-street, W.
1885. Roberts, Edward Coleridge, M.R.C.S., Southgate, N.
1874. Roberts, Frederick Thomas, M.D., 102, Harley-street, W. p, vp 2, o, ll, c. *Councillor.*
1884. Robinson, Arthur Henry, M.D., St. Mary's Infirmary, Highgate-hill, N.
1897. Robinson, Henry Betham, M.S., 1, Upper Wimpole-street, W.
1896. Robson, A. Mayo, F.R.C.S., 7, Park-square, Leeds.
1847. *Rogers, William Richard, M.D., 26, Upper Baker-street, N.W. vp, c 6.
1897. Rolleston, Humphry Davy, M.D., 55, Upper Brook-street, W. c 2. *Councillor.*
1890. Root, Arthur Guernsey, M.D., 46, Eagle-street, Albany, New York, U.S.A.
1886. Rose, Robert Duncan, F.R.C.S., 8, St. Leonards, York.
1874. Rose, William, F.R.C.S., 17, Harley-street. c 2, ll, vp 2.
1883. *Ross, Daniel McClure, M.D., Cedar Lodge, Littledown-road, Bournemouth.
1888. *Roth, Bernard, F.R.C.S., J.P., 38, Harley-street, W.
1893. Roughton, Walter, F.R.C.S., Cranborne House, New Barnet, Herts.
1881. *Routh, Amand, M.D., 14a, Manchester-square, W. c 5, s 2.
1848. *Routh, Charles Henry Felix, M.D., 52, Montagu-square, W. p, vp 2, o, ll, s 4, c 6, sm. *Trustee.*
1891. Ruffer, Marc Armand, M.D., Medical School, Cairo, Egypt.
1887. Rushworth, Frank, M.D., "Langdale," Goldhurst-terrace, South Hampstead, N.W.
1889. Russell, James Samuel Risien, M.D., 4, Queen Anne-street, W.
1886. Rutherfoord, Henry Trotter, M.D., Salisbury House, Taunton.

1887. Sainsbury, Harrington, M.D., 63, Welbeck-street, W.
1863. *Sansom, Arthur Ernest, M.D., 84, Harley-street, W. p, vp, s 2, c 6, sm, §, ll, o.

1895. Sansom, Charles Lane, F.R.C.S. Edin. (*address uncommunicated*).
1897. Sansom, Harry Arthur, M.D., The Glen, West-end-lane, West Hampstead, N.W.
1886. Savage, George Henry, M.D., 3, Henrietta-street, W. c.
1886. Savill, Thomas Dixon, M.D., 60, Upper Berkeley-street, W.
1899. Sayres, Alexander Ward Fortescue, M.D., Lansdown House Solway-hill, Woodford Green, N.E.
1899. Schorstein, Gustave, M.B., 11, Portland-place, W.
1873. Sedgwick, James, M.D., The Firs, Copse-hill, Wimbledon.
1868. Sedgwick, Leonard William, M.D., 48, Gloucester-terrace Hyde Park, W. vp 2, c 4, § 3.
1899. Segundo, Charles Sempill de, M.B., 6, Brook-street, Hanover-square, W.
1883. Semon, Sir Felix, M.D., 39, Wimpole-street, W. §, c.
1887. Servais, Leopold, M.D., Antwerp, Belgium.
1889. Shaw, George, M.B., 1, The Drive, West Brighton.
1884. Shaw, John, M.D., 32, New Cavendish-street, Cavendish-square, W.
1886. Sheild, Arthur Marmaduke, F.R.C.S., 4, Cavendish-place, W. c 2, s 2.
1890. Sheppard, William John, M.D., 211, Upper Richmond-road, Putney, S.W.
1885. Shoemaker, John V., M.D., 1031, Walnut-street, Philadelphia, U.S.A.
1894. Shuttleworth, George Edward, M.D., Ancaster House, Richmond, Surrey.
1890. Silk, John Frederick William, M.D., 47, Devonshire-street, W.
1884. Simpson, James Herbert, M.D., Hillmorton-road, Rugby, Warwickshire.
1898. Simpson, John Pollock, M.D., 1, Blandford-street, Manchester-square, W.
1884. Sinclair, John, M.R.C.P., General Post Office, St. Martin's-le-Grand, E.C.
1891. Sisley, Richard, M.D., 14, Park-lane, W.
1883. *Skerritt, Edward Markham, M.D., Richmond Hill, Clifton.
1886. Slater, Charles, M.B., 81, St. Ermin's-mansions, Westminster, S.W.
1862. Slight, George, M.D., 14, Old Burlington-street, W. c 2.
1889. Smale, Morton, M.R.C.S., 22a, Cavendish-square, W. c 3.
1845. *Smiles, William, M.D., St. Martha's Lodge, Guildford. vp 2, s 4, c 9, sm.
1897. Smith, A. Lionel H., M.R.C.S., Addenbrooke's Hospital, Cambridge.
1896. Smith, E. Stanley, M.D., 68, Wimpole-street, W.
1887. Smith, Frederick John, M.D., 138, Harley-street, W. c. *Councillor.*
1873. Smith, Heywood, M.D., 18, Harley-street, W. c 3.
1880. Smith, Noble, F.R.C.S. Edin., 24, Queen Anne-street, W.
1891. Smith, Solomon Charles, M.D., Four Oaks, Walton-on-Thames.
1877. Smith, Sydney Lloyd, M.R.C.S., 25, Argyle-square, King's Cross, W.C.

1882. Smith, Thomas Frederick Hugh, F.R.C.S., Farningham, Kent.
1873. *Smith, Thomas Gilbart, M.D., 68, Harley-street, W. vp 2, s 2, sm, c 4. *Trustee.*
1872. Smith, Walter, M.R.C.P. Edin., Orleans House, 60, Regent's Park-road, N.W.
1899. Smithson, Oliver, M.R.C.S., Hilldrop House, Grays, Essex.
1874. Smyth, William Woods, L.R.C.P. Edin., Maidstone.
1894. Snape, Ernest Alfred, M.D., 39, Welbeck-street, W.
1893. Spencer, Herbert Ritchie, M.D., 104, Harley-street, W. c.
1887. Spicer, Scanes, M.D., 28, Welbeck-street, W.
1883. Spitta, Edmund Johnson, M.R.C.S., Ivy House, Clapham Common, S.W.
1864. Squire, Alexander John Balmanno, M.B., 24, Weymouth-street, Portland-place.
1881. Startin, James, M.R.C.S., 15, Harley-street, W.
1892. Staveley, William H. C., F.R.C.S., 13, South Eaton-place, S.W.
1896. Steele-Perkins, George Chapman, M.B., 85, Wimpole-street, W.
1884. Stephens, William John, L.S.A., 9, Old Steine, Brighton.
1882. Stewart, James, F.R.C.P. Edin., Dunmurry, Sneyd Park, near Clifton.
1894. Stewart, Kenneth Trevor, M.D., 26, Manchester-square, W.
1883. Stewart, William Robert Henry, F.R.C.S. Edin., 42, Devonshire-street, Portland-place, W.
1884. Stiven, Edward Winnan Fleming, M.D., Lincoln House, Harrow, Middlesex.
1885. Stivens, B. H. Lyne, M.D., 107, Park-street, Grosvenor-square, W.
1848. *Stocker, John Sherwood, M.D., 2, Montagu-square, W. c 10, s 2.
1884. Stoker, George, M.R.C.S., J.P., 14, Hertford-street, Mayfair, W.
1892. Stonham, Charles, F.R.C.S., 4, Harley-street, W.
1877. Stowers, James Herbert, M.D., 128, Harley-street, W.
1873. Strange, William Heath, M.D., 2, Belsize-avenue, Hampstead, N.W. c 3.
1881. Sturge, William Allen, M.D., Maison Malausséna, Boulevard Dubouchage 29, Nice. sm.
1889. Sumpter, Walter John Ernely, M.R.C.S., Sheringham, Norfolk.
1892. Sunderland, Septimus, M.D., 11, Cavendish-place, W.
1876. *Sutherland, Henry, M.D., 21, New Cavendish-street, W.
1896. Swanton, James Hutchinson, M.D., 40, Harley-street.
1892. Swift, William John Cropley, M.R.C.S., 4, Gordon-square, W.C.
1885. *Syers, Henry Walter, M.D., 75, Wimpole-street, W.
1884. Symonds, Horatio Percy, F.R.C.S., 35, Beaumont-street, Oxford.
1893. Symonds, Charters James, F.R.C.S., 58, Portland-place, W. c.
1899. *Sympson, Edward Mansel, M.D., Deloraine Court, Lincoln.

1864. Tait, Edward Wilmshurst, M.R.C.S., 48, Highbury-park, N.
1875. Tamplin, Charles Harris, M.R.C.S., 17, Paragon, Ramsgate.

1882. Taylor, Seymour, M.D., 16, Seymour-street, Portman-square, W. c 2.
1897. Templeton, George, F.R.C.S., 10, Seymour-street, Portman-square, W.
1898. Thomas, John Lynn, F.R.C.S., 28, Charles-street, Cardiff.
1859. *Thompson, Edmund Symes, M.D., 33, Cavendish-square, W. vp, o, s 3, c 3, sm.
1855. *Thompson, Sir Henry, Bart., F.R.C.S., 35, Wimpole-street, W. vp, ll, c 4.
1896. Thompson, Henry George, M.D., 86, Lower Addiscombe-road, Croydon, Surrey.
1895. Thompson, James Andrew Baird, M.D., White Hall, Abridge, Essex.
1873. Thomson, John Roberts, M.D., Monkchester, Bournemouth, Hants.
1894. Thomson, St. Clair, M.D., 28, Queen Anne-street, W.
1898. Thorne, Atwood, M.B., 10, Nottingham-place, W.
1899. Thorne, W. Bezly, M.D., 53, Upper Brook-street, W.
1897. Thornton, Bertram, M.R.C.S., J.P., Berkeley Lodge, Trinity-square, Margate.
1892. Thornton, George, M.D., Fountain Hospital, Lower Tooting, S.W.
1876. Thornton, John Knowsley, M.C., J.P., Hildersham Hall, Cambs, p, vp, c 3.
1856. *Thudichum, John Louis William, M.D., 11, Pembroke-gardens, Kensington, W. vp, ll, o, c.
1884. Thursfield, Thomas William, M.D., J.P., Selwood, Beauchamp-square, Leamington.
1865. Travers, William, M.D., 2, Phillimore-gardens, Kensington, W.
1897. Tresilian, Frederick James, M.D., Redlingtons, Enfield.
1884. *Treves, Frederick, F.R.C.S., 6, Wimpole-street, W. c 3, ll, vp 2.
1899. Tubby, Alfred Herbert, M.S., 25, Weymouth-street, W.
1898. Tucker, E. F. Gordon, M.R.C.S., Indian Medical Service, care of Messrs. H. King and Co., 45, Pall Mall, S.W.
1882. Tuke, Charles Molesworth, M.R.C.S., Chiswick House, Chiswick.
1886. Tuke, Thomas Seymour, M.B., Chiswick House, Chiswick.
1884. Turner, George R., F.R.C.S., 41, Halfmoon-street, Piccadilly, W. vp 2, s 2, c 3. *Councillor.*
1890. Tweed, Edward Reginald, M.D., Hembury Fort, Honiton, Devon.
1883. Tweedy, John, F.R.C.S., 100, Harley-street, W. c.
1891. Tyson, William Joseph, M.D., 10, Langhorne-gardens, Folkestone.

1887. *Underwood, Edward T., M.D., Fort Bombay, India.

1897. Vassie, Alexander Henry, M.B., 98, Priory-road, West Hampstead, N.W.
1883. Venning, Edgcombe, F.R.C.S., 30, Cadogan-place, S.W.
1899. Vise, Christopher, M.D., 39, Mount Pleasant-road, Tunbridge Wells.
1893. Voelcker, Arthur Francis, M.D., 31, Harley-street, W. c. *Hon. Secretary.*

1892. Waggett, Ernest Blechynden, M.B., 45, Upper Brook-street, W.
1850. *Waggett, John, M.D., Perivale, Bournemouth.
1884. Wakley, Thomas, Jun., L.R.C.P. Lond., 5, Queen's-gate, S.W.
1850. *Wakley, Thomas Henry, F.R.C.S., 5, Queen's-gate, S.W.
1898. Wallace, Cuthbert Sidney, F.R.C.S., St. Thomas's Hospital, S.E.
1894. Wallis, Frederick Charles, F.R.C.S., 26, Welbeck-street, W. c. *Hon. Secretary.*
1880. Walsham, William Johnson, F.R.C.S., 77, Harley-street, W. c.
1895. Walters, Frederick R., M.D., 21, Wimpole-street, W.
1899. Ward-Humphreys, George Herbert, M.R.C.S., 9, Cavendish-place, W.
1894. Waring, Holburt Jacob, F.R.C.S., 9, Upper Wimpole-street, W.
1899. Washbourn, John Wickenford, M.D., 6, Cavendish-place, W.
1894. Waterhouse, Herbert Furnivall, F.R.C.S., 81, Wimpole-street, W. *Councillor.*
1883. Waterhouse, William Dakin, LL.D., 50, West-end-lane, West Hampstead, N.W. c.
1889. Waugh, Henry Dunn, M.D., 6, Sumner-place, Onslow-square, S.W.
1884. Webb, F. Ernest, M.R.C.S., 113, Maida-vale, W.
1896. Weber, Frederick Parkes, M.D., 19, Harley-street, W.
1889. Weber, Sir Hermann, M.D., 10, Grosvenor-street, W.
1898. Weeks, Courtenay Charles, M.R.C.S., 9, Lewisham-park, S.E.
1838. *Wells, John Robinson, F.R.C.S., 4, Pierrepoint-road, Acton, W. c 2.
1884. West, Samuel, M.D., 15, Wimpole-street, W. s 2, c 4, cfc, ll. *Vice-President.*
1889. Wethered, Frank Joseph, M.D., 83, Harley-street, W.
1892. Wheaton, Samuel Walton, M.D., 76, The Chase, Clapham Common, S.W.
1882. Whipham, Thomas T., M.D., 11, Grosvenor-street, W. sm, c.
1899. White, Charles Percival, M.B., 22, Cadogan-gardens, S.W.
1889. White, E. F., F.R.C.S., Westlands, 280, Upper Richmond-road, Putney, S.W.
1880. *White, William Henry, M.D., 43, Weymouth-street, W. c. 3.
1883. Whitehead, Walter, F.R.C.S. Edin., F.R.S.E., 499, Oxford-road, Manchester. c.
1885. Whitla, William, M.D., 8, College-square North, Belfast, Ireland.
1877. Whitmore, William Tickle, F.R.C.S. Edin., 7, Arlington-street, Piccadilly, W.
1898. Willey, F. Ingor, M.B., The Wych, 8, Avenue-road, Highgate, N.
1872. Williams, Charles Theodore, M.D., 2, Upper Brook-street, Grosvenor-square, W. p, vp 2, ll, s 2, sm, o, l 3, c 9.
1876. Williams, Henry William, M.D., Hillside, Guilsborough, Northampton.
1883. Williams, Sir John, Bart., M.D., 63, Brook-street, W. c 3.
1900. Willock, Edward Hulse, M.R.C.S., 113, London-road, Croydon.

1873. Wills, Thomas Munns, F.R.C.S.I., J.P., 44, Merton-road, Bootle, Liverpool.
1893. Wills, William Alfred, M.D., 29, Lower Seymour-street, W.
1892. Wilson, Claude, M.D., Belmont, Tunbridge Wells.
1884. Winslow, H. Forbes, M.D., 14, York-place, Portman-square, W.
1873. Winslow, Lyttleton Stewart Forbes, M.B., D.C.L., LL.D., 33, Devonshire-street, W. c.
1876. Woakes, Edward, M.D., 78, Harley-street, W.
1900. Wood, Guy M., M.B., 9, Endsleigh-street, W.C.
1886. Wood, T. Outterson, M.D., 40, Margaret-street, Cavendish-square, W. c 3.
1873. Woodhouse, Robert Hall, M.R.C.S., 1, Hanover-square, W.
1891. Woollett, Charles Jerome, F.R.C.S., "Ambleside," Streatham, S.W.
1898. Wright, Sydney Faulconer, M.D., St. Peter's Lodge, Eltham-road, Lee, S.E.
1884. Wyman, William Sanderson, M.D., Red Brae, Putney-hill, S.W. c 2.

1891. Yarr, Michael Thomas, Major, R.A.M.C., "The Siamese Legation," 23, Ashburn-place, S.W.
1884. Yeo, I. Burney, M.D., 44, Hertford-street, Mayfair, W.
1897. Young, Adam, M.R.C.S., 2, College-crescent, South Hampstead, N.W.
1897. Young, Williams H. Frome, M.R.C.S., Malcolm Corner, Worple-road, Wimbledon.
1884. Younger, Edward George, M.D., 19, Mecklenburgh-square, W.C. c 2. *Councillor.*

NON-SUBSCRIBING FELLOWS.

1868. Bateman, Sir Frederic, M.D., J.P., Upper-street, Giles-street, Norwich.
1872. Bell, John Hougham, M.D., Ventnor, Isle of Wight.
1868. Buckle, Fleetwood, M.D., Staff Surgeon, R.N., Merton Lodge, Southsea.
1870. Clouston, Thomas Smith, M.D., Royal Asylum, Morningside, Edinburgh. FM 1870.
1868. Folker, William Henry, F.R.C.S., Hanley, Staffordshire.
1869. Foster, Sir Walter B., M.D., M.P., 14, Temple-row, Birmingham.
1868. Fox, Charles Henry, M.D., 35, Heriot-row, Edinburgh.
1868. Gaine, Charles, 30, Gay-street, Bath.
1871. Glynn, Thomas Robinson, M.D., 62, Rodney-street, Liverpool.
1868. Knaggs, Samuel, 2, Bradley-lane, Huddersfield.
1869. Lees, Charles Alexander, M.D., Dep. Inspector-General, R.N.
1869. Mathews, Robert, Bickley, Kent.
1871. Maurice, Oliver Calley, M.D., J.P., 75, London-street, Reading.
1868. McIntyre, John, M.D., LL.D., Odiham, Hants.
1868. Nevins, John Birkbeck, M.D., 32, Princes-avenue, Liverpool.
1871. Ogle, William, M.D., The Elms, Derby.
1869. Philipson, Sir George Hare, D.C.L., M.D., J.P., 7, Eldon-square, Newcastle-on-Tyne.
1869. Price, William Preston, M.D., 1, Ethelbert-crescent, Margate.
1869. Prior, Charles Edward, M.D., 4, Goldington-road, Bedford.
1869. Roberts, Bransby, M.D., Ash Grove, Eastbourne.
1869. Stear, Henry, Saffron Walden, Essex.
1869. Taylor, Charles Bell, M.D., 9, Park-row, Nottingham.
1869. Walker, John Swift, M.D., Hanley, Staffs.
1868. Wiblin, John, F.R.C.S., Wimborne, Dorset.

*** As it is very desirable that the foregoing Lists should be kept as accurately as possible, Fellows are requested to send notice of any corrections that may be necessary to the Secretaries or to the Registrar.

GENERAL MEETING.

May 14*th*, 1900.

FREDERICK T. ROBERTS, M.D., *President*, in the Chair.

Report of the Council for the Session, 1899–1900.

THE Council begs to report that the work of the Session has been successful; the meetings have been well attended, the papers read and the cases shown have been of the usual high merit.

The total number of Fellows is now 729, whereas a year ago it was 747. The details for the past three years are shown in the following table:—

	1897–1898.			1898–1899.			1899–1900.		
	Ord.	Hon., &c.	Total.	Ord.	Hon., &c.	Total.	Ord.	Hon., &c.	Total.
At beginning of Session	655	109	764	659	87	746	662	85	747
Elected in Session ...	30	—	30	32	6	38	21	1	22
	685	109	794	691	93	784	683	86	769
Less—									
Deaths	9	22	31	7	8	15	10	7	17
Resignations	10	—	10	12	—	12	14	—	14
Struck off for non-payment of subs.	7	—	7	10	—	10	9	—	9
At end of Session ...	659	87	746	662	85	747	650	79	729
Increase or Decrease ...	+4	−22	−18	+3	−2	+1	−12	−6	−18

Sir Joseph Fayrer, Bart., a former President, was elected an Honorary Fellow.

The Fellows whose deaths the Council has to deplore are as follows:—Four Honorary—Sir James Paget, Professors Halla, Lombard, and Nussbaum; three Foreign Corresponding—Drs. De Muynck, Hyman, and Journez; 10 Ordinary—Mr. W. Adams (President in 1857), Mr. G. H. Bailey, Mr. J. W. Barnes, Dr. S. Hyde, Dr. Norman Kerr, Dr. Cooper Key, Mr. A. C. Routh, Mr. Lawson Tait, Dr. McNeill Whistler, and Mr. Joseph White.

The Annual Oration, delivered by Mr. Alban Doran, entitled "Shakespeare and the Medical Society," was very largely attended and highly appreciated.

The Lettsomian Lectures, "Practical Observations on Cancer of the Breast," given by Sir William Mitchell Banks, were a notable contribution to an important subject.

The Anniversary Dinner, with the President in the Chair, was held as usual on March 8th, the birthday of John Fothergill. In every respect the Dinner was a complete success.

The President, on the award of the Council, presented the Silver Medal of the Society to Dr. W. H. Allchin, who had held the office of Honorary Librarian for more than 17 years.

(Signed) Frederick T. Roberts,
President.

Report of the House and Finance Committee for the Session, 1899–1900.

The Committee have to report that the income derived from tenants has been well maintained during the year, and that a first and final dividend of £33 12*s.* 7*d.* (less £4 2*s.* 0*d.* legal expenses) has been received from the Trustees of the Society's freehold property in Bolt Court in settlement of their claim against the estate of a late tenant.

Since the date of the last report two more debentures have been drawn and paid off, thus reducing the amount outstanding to £2,800.

It is proposed during the autumn to have the external wood and iron work of the Society's leasehold premises, as well as the rooms occupied by the Registrar, cleaned and repainted; when this has been done the premises will be in a good state of repair.

The Committee have also to report that a judicial decision has been pronounced in favour of the Society with regard to certain restrictions, proposed to be inserted by the lessor, in the new lease to which the Society is entitled under the terms of the agreements with Mr. Boyce.

(Signed) D. H. Goodsall,
Chairman.

Honorary Librarian's Report, Session 1899–1900.

I have the honour to report that the condition of the Library for the year ending April 30th, 1900, has been satisfactory. The number of volumes borrowed from the Library has been 195, somewhat fewer than last year. The subscription with Lewis's Library has been found of great convenience, since it allows books, asked for by Fellows and not in the Library, to be obtained at short notice; 65 volumes have during the year been obtained from Lewis's Library.

The additions to the Library during the year, exclusive of periodicals and exchanges, are: purchased, 15 volumes; presented by authors and publishers, 12 volumes; second-hand volumes presented, 7; making a total of 34 volumes. The principal donors are Drs. W. H. Allchin,

Mitchell Bruce, Alex. Morison, Samuel West, A. Haig, Frederick J. Smith, Heywood Smith, John Lowe, and Mr. Reginald Harrison.

The rebinding of the earlier works in the Library is proceeding slowly, owing to the small funds at the disposal of the Library Committee for the purpose. During the year 16 volumes have been rebound.

Appended is a list of the principal new works acquired.

(Signed) SIDNEY MARTIN,
Hon. Librarian.

Among the books added to the Library during the Session were the following :—

ALLBUTT (C.). 'System of Medicine.' Vols. vii and viii.
ALLCHIN (W. H.). 'Manual of Medicine.' Vol. i.
BRUCE (Mitchell). 'Principles of Treatment.'
CHEYNE (W.) and BURGHARD (F.). 'Manual of Surgical Treatment.' Parts i and ii.
HAIG (A.). 'Diet and Food.' 2nd edition.
HARE (H. Amory). 'Progressive Medicine.' Vols. i–iv, 1899.
HARRISON (Reg.). 'Stone, Prostate, and other Urinary Disorders.
LUFF (A. P.) and PAGE (F.). 'Chemistry for Medical Students'
MILLS (C. K.). 'Nervous System and its Disorders.'
MORISON (A.). 'Nervous System and Visceral Disease.'
MUIR (R.) and RITCHIE (J.). 'Manual of Bacteriology.' 2nd edition.
NEALE (R.). 'Appendix to Digest.' 1891–99.
OSLER (W.). 'Principles and Practice of Medicine.' 3rd edition.
SMITH (Eustace). 'Wasting Diseases of Infancy.' 6th edition.
SMITH (Fred. J.). 'Differential Diagnosis.'
SMITH (Heywood). 'Practical Gynæcology.' 2nd edition.
WEST (Samuel). 'Granular Kidney and Physiological Albuminuria.'
WHITE (W. Hale). 'Materia Medica.' 4th edition.

MEDICAL SOCIETY

DR. BALANCE SHEET, FROM 1ST OCTOBER,

RECEIPTS.

			£ s. d.
Balance from Last Account			104 12 1
Ordinary—		£ s. d.	
Subscriptions		508 14 6	
Entrance Fees		23 2 0	
Life Composition Fee		12 12 0	
Rents { Bolt Court *	£116 10 7		
Chandos Street	722 14 6		
Occasional Lettings ..	41 9 6		
		880 14 7	
* Including £29 10*s.* 7*d.* div. (less legal expenses) on late tenant's arrears.			
Sale of 'Transactions'		2 5 0	
Interest on Deposit Account		0 12 9	
			1,428 0 10
Extraordinary—			
Subscriptions from Fellows towards purchase of a Magic Lantern			16 4 0
			£1,548 16 11

FOTHERGILLIAN

	£ s. d.
Balance from Last Account	57 11 1
Dividend on £916 10*s.* 5*d.*, 2¾ % Consols	25 4 0
	£82 15 1

Examined, compared with vouchers, and found correct, October 2nd, 1900.

(Signed) W. J. HADLEY,
HERBERT F. WATERHOUSE, } *Auditors.*

OF LONDON.

1899, TO 30TH SEPTEMBER, 1900. CR.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Ordinary—						
Rent, 11 and 12, Chandos Street	290	0	0			
Rates, Taxes, and Insurance	157	10	10			
Registrar's Salary and Allowances	120	8	0			
Collector's Poundage on Subscriptions	25	8	8			
Reporter's Salary	22	1	0			
Stationery and Printing	15	13	1			
Printing and issuing 'Transactions'	131	4	9			
Library Expenses	32	13	10			
Postage and Stamps	10	9	9			
Coals and Wood	9	17	2			
Gas and Electric Supply	46	9	6			
Repairs	13	12	11			
Interest on Debentures	110	7	8			
Conversazione	45	0	0			
Refreshments at Meetings	14	0	6			
Patients' Expenses, Clinical Meeting	1	14	6			
Lantern Demonstrations	2	2	0			
Bankers' Charges, 1*s.* 3*d.*; Travelling, 5*s.*	0	6	3			
Silver Medal	1	10	0			
Wages	46	13	6			
Chandlery and Cleaning	12	18	2			
Anniversary Dinner £86 18 6						
Less Fellows' Tickets 71 18 6						
	15	0	0			
				1,125	2	1
Extraordinary—						
Redemption of Two Debentures				200	0	0
Balance at Bank, 30th September, 1900—						
Deposit Account	100	0	0			
Current Account	123	14	10			
				223	14	10
				£1,548	16	11

FUND.

	£	s.	d.
Books Purchased for Library	10	0	10
Balance at Bank, 30th September, 1900	72	14	3
	£82	15	1

(Signed) D. H. GOODSALL,
Treasurer.

TRANSACTIONS

OF THE

MEDICAL SOCIETY OF LONDON.

127TH SESSION.

October 9th, 1899.

OPENING ADDRESS ON THE PROGRESS OF MEDICINE IN THE NINETEENTH CENTURY.

By the President, FREDERICK T. ROBERTS, M.D., F.R.C.P. Lond.

GENTLEMEN,—I imagine that it would be almost impossible for anyone to whose lot it falls at the present time to give an address on an occasion such as this to resist the temptation of taking as his text, from some aspect or other, the century now so rapidly drawing to its close. The remarkable developments, changes, and revolutions which it has witnessed, the wonderful discoveries and inventions to which it has given birth, with their bearing and influence upon the numerous problems of practical life, upon the advance of so-called civilisation, and upon the progress and well-being of humanity, afford boundless scope for any amount of "talk" on all kinds of topics. "Medicine"—using the word in its comprehensive sense, as including all the branches of the "healing art"—opens up an inexhaustible field from this point of view, and several addresses have been recently delivered bearing upon the subject. At the inaugural meeting of what many regard as the last session of the Medical Society of London in the nineteenth century, it would be manifestly absurd and inhuman to expect the President not to take advantage of such an opportunity for selecting a topic ready to hand, in spite of previous utterances. At any rate, I cannot resist the temptation, and must ask you to bear with me while I endeavour to indicate, on very general lines, what seem to me to be the more striking

changes which have affected our profession in this country during the present century, and the main results in the direction of progress which have been achieved.

In the abstract a hundred years seems a very long time, and under circumstances of ordinary humdrum existence no doubt they pass slowly enough; while only a very small proportion of the human race have up to the present era attained this degree of longevity, though we are promised wonderful improvements in this respect in the future. The earlier portion of the century must, I imagine, have been pretty dull and wearisome to the mass of people; but I have a strong impression that its latter half, at any rate, has flown by with astonishing rapidity in the experience of a considerable number of persons who have been personally engaged and taken an active part in the march of events, or who have even watched their progress with more or less lively interest. To this category have belonged a very large and constantly-increasing proportion of the members of the medical profession, and there are some still in our midst who have witnessed almost from the first the more important changes which have marked its history. I fancy that with most of them the years have sped by only too quickly, and certainly such has been my own experience. I speak somewhat feelingly, for, if you will excuse a personal reference, I am forcibly reminded to-night of the disquieting fact that I have been associated with the profession for very close on half the century, and it seems but as yesterday that I started on my medical career as an apprentice in a respectable, quiet, sleepy, out-of-the-world town in South Wales, little dreaming that I should ever dare to enter upon the fierce conflict of professional life in London, and still less that I should be thought worthy to occupy the proud and honourable position in which, by your generous kindness, I now find myself.

The reminiscence to which I have just referred naturally calls attention to a most important change which has taken place in relation to our profession during this century, namely, the doing away entirely with the apprenticeship system. At the time of which I speak an apprenticeship of five years' duration was an essential part of the curriculum—at least, in the case of those preparing for general practice. According to my indentures, still in my possession, I was bound for seven years at the precocious age of nine years, and actually did make a start when 12 years

old! To most of you, probably, this is a mere matter of ancient history, or perhaps some of you have never even heard of a mode of introduction to, and preparation for, the medical profession which in this "very superior" age you might be inclined to regard as an unpleasant reminder of "trade," and as the remnant, not only of an unscientific era, but one sadly deficient in culture and "good form." It would serve no practical purpose to discuss to-day whether apprenticeship was useful or the reverse, for, so far as I can see, it has gone never to return; but I cannot allow the century to close over its grave without a parting word of kindly appreciation. No doubt the absurdly prolonged apprenticeship was a great mistake and a waste of precious time; and even when of moderate length it often involved degrading and menial drudgery, or was a mere farce. Sometimes, indeed, it was worse, for it afforded opportunities for "jobbery" of the most offensive type, having been utilised as a means of foisting men into prominent hospital or other appointments to which they had no real claim, and for which they might be entirely unfitted. Nevertheless, I firmly believe that this method of initial or preliminary training for the medical profession had decided advantages within rational limits, and served a most useful purpose in its day. Personally, I have never regretted that it fell to my lot to have to pass through such an experience; and I acknowledge with gratitude that the practical lessons impressed upon my mind at that time not only helped me as a student, but have served me in good stead on many and many an occasion since.

Passing from the negative or destructive to the positive or constructive, I presume I shall be echoing the sentiments of all here present when I affirm that the medical profession has made vast strides in the direction of true progress during the past century, especially its latter half, in spite of what carping and unfriendly critics may say to the contrary. Perhaps there is a tendency sometimes to boast a little too loudly of what has been achieved, and to speak or write in somewhat extravagant and needlessly glowing terms of the glories of the past and the brilliant promises of the future. "Change" is by no means always synonymous with "progress," and we may as well frankly acknowledge at the outset that the changes affecting the profession which have come within our own experience have not invariably been along the line of real and lasting progress. Most of us must

be able to call to mind passing events or episodes, wonderful discoveries, epoch-making methods of treatment, and the like, which have had their little day and then sunk into oblivion. Some of these disturbers of the peaceful current of events, indeed, we are only too glad to forget, as having for the time brought discredit, or even ridicule, upon our noble calling. On the whole, however, the balance is immensely on the right side, and we may fairly claim that medicine occupies to-day a position of which it has no cause to be ashamed. The advances it has made during this century are not only remarkable in extent and highly beneficial in their results, but are in the main established on a solid and enduring foundation, upon which those who take up the work in the coming century may go on building indefinitely, to the incalculable advantage of future generations.

Starting, then, from this general assumption of the undeniable progress of the medical profession, my main object this evening is to endeavour to indicate briefly what, according to my judgment, are its most striking features, and to sum up concisely some of the more important practical results which have been achieved. Before doing so, however, I must call attention for a moment, without entering into any details, to the means or agencies by which these results have been brought about, which will suggest many interesting facts in relation to the history of medicine during this century, and the more obvious changes which have affected the profession.

We must not fail to recognise at the outset the influence of the remarkable advance in general education and culture, which has told upon the profession of medicine in common with others, and assures, at any rate, that those who desire to enter its ranks shall have acquired the rudiments of general knowledge, and have had some preparatory training for its special studies, while a fair proportion may now be regarded as "highly educated." Then, whatever may have been the actual state of things previously, the nineteenth century may be credited, to all intents and purposes, with the establishment of a comprehensive and rational system of compulsory education and training for all who aim at becoming medical practitioners, and its progressive development on scientific and practical lines, the latter including that most important branch, "clinical teaching," in accordance with the requirements of the

age. Within recent years, I need scarcely say, this development has been going on by "leaps and bounds," with proportionate prolongation of the period of training. What things are going to end in I have no idea; but I do feel a sort of condolence with, and sympathy for, the "medical student" of the future, or perhaps rather for his unfortunate parents or guardians. Still, my sympathy may be quite misplaced, for not a few modern students do not seem to be at all satisfied with, or to have had anything like enough of, tuition during their curriculum, as evidenced by the frantic eagerness and craving exhibited nowadays for so-called "post-graduate teaching," sometimes even on the most elementary subjects.

A most important change or development, which deserves special notice, is the adoption of the scientific spirit, if I may so express it, and of scientific methods in relation to the study and teaching of medical subjects; as well as the introduction into the curriculum of allied subjects more strictly scientific, and the promotion and encouragement of their methodical and deeper study. Apart from the acquisition of the scientific knowledge and skill—in physiology, chemistry, and other branches—which to-day is justly regarded as essential for any properly educated and competent medical practitioner, I need scarcely remind you that those who teach these and kindred subjects attach great importance to the preliminary scientific training which their study involves, as a means of preparation for the active duties and practical work of the medical profession. That in the main this scientific development has been of immense advantage cannot for a moment be doubted; but, though I know it is rank heresy to suggest such a thing, may it not be just possible that the "preliminary scientific" business, at any rate, has been worked for all it is worth, unless it be intended as a barrier to keep out some of the horde who still strive to invade our overcrowded profession! The more advanced scientific training now in vogue for special purposes stands on a different footing, and undoubtedly deserves every attention.

Another factor which has manifestly contributed in an incalculable degree to the progress of medicine has been the separation or unravelling, if I may so speak, of subjects which, though essentially distinct, were formerly, if dealt with at all, included under one term and discussed in a single course of lectures,

often short and necessarily incomplete; and the addition of new subjects the outcome of increasing knowledge, either as part of the medical curriculum or as fields for subsequent study. Imagine the time, not so very far distant, when there was no independent instruction in, or separate study of, pathology and morbid anatomy, materia medica and therapeutics, etiology and hygiene, but all were muddled together under "Lectures on Medicine." Taking a comprehensive view of the matter, of course the broad division of medical subjects was an unmixed gain, and its advantages cannot be over-estimated; but here, again, things may be carried too far, and there is, perhaps, a tendency nowadays to multiply subjects needlessly The number which the medical student has to grapple with is certainly sufficiently formidable, and I fancy he would be inclined to cry "Hold, enough!" But here, also, comes in the thorny question of "specialism," about which I will only venture to express the opinion that to this comparatively modern development the progress of medicine owes more than can be imagined or expressed in words, and that when carried out legitimately and rationally it has proved, and always will prove, of infinite advantage to the profession and to humanity. It is its abuse which works such serious mischief, and against which we must ever protest.

It may be a matter of controversy how far the founding and development of examinations in the United Kingdom in relation to the medical profession have contributed to its advance; but, while fully recognising the fact that things are, even at the present time, anything but satisfactory, no one can deny that they have had no small share in promoting the general progress. The history of these examinations, both before and during this century, would in itself afford ample material for an address, and it is decidedly entertaining. Time was when the mass of general practitioners had no diploma at all, and for long the examinations were absurdly inadequate and often a farce. Steady improvement, however, has taken place, more especially during the last 40 years, and for this more or less credit must be given to the General Medical Council. As things now exist, I think we can claim for examinations these advantages:—First, most of them are sufficiently comprehensive and searching to guarantee that those who obtain degrees or diplomas or licences to practise are,

at any rate, fairly competent for the duties that lie before them; secondly, some of them, either in themselves or by their associated "honours" examinations, stimulate the mental energies and zeal of those who aim at the higher positions in the profession, while at the same time they in a general way determine or regulate, so to speak, the status of those who desire to devote themselves more particularly to one of its main divisions, such as Medicine, Surgery, Gynæcology, or Public Health; and, thirdly, it cannot be doubted for a moment that the progressive improvement in the examinations has had a material influence in initiating and stimulating advance in medical education and bringing it up to the required standard. Let us hope that in the coming century these two factors working together may raise the medical vocation to that still higher position to which it is entitled, and may ensure that no individual can enter its ranks who is not thoroughly trained and qualified, and fully competent to engage in its great work, whatever sphere of labour he may select.

Having spoken about medical education and examinations, a word must be said about the Institutions which have been concerned with, and responsible for, these important departments. I do not propose to criticise the old Corporations and Medical Schools, but I think it will be generally acknowledged that at one time they were hardly up to the mark, and it was a good thing for the profession when circumstances made them "sit up" a bit. The establishment of additional Schools of Medicine, each doing its best to bring students up to the required standard, to "keep up with the times," and to provide the means for education and training which modern science demands, has undoubtedly been of conspicuous service, by promoting and ensuring a healthy rivalry and competition; but whether, in London at any rate, things are not in danger of being overdone in this direction may be a matter of opinion. And here I may conveniently draw attention to a most significant change affecting the profession which has happened during the nineteenth century, and that is the admission of "Lady Doctors" into our ranks. They have now a "School" of their own, and it must be cordially acknowledged that they train their students admirably, who often compete most successfully with the male students. It is not for me to say whether or not this change has been a progressive step—time will show; but in the meanwhile they decidedly hold

their own, and even those gentlemen who shake their heads ominously can do nothing but accept the situation. Personally, I bear the lady practitioners no ill-will, and can testify that many of them are highly gifted, conscientiously devoted to their work, and fully deserving of our respect and esteem. In relation to medical education, I must not omit to mention the provincial medical schools, many of which have done excellent work—some, indeed, being to-day so splendidly equipped that they excite the admiration and envy of the metropolitan schools. Here, too, I may refer to the founding of so-called modern Universities, which are the growth of this century; and it would not be just if I did not mention specially the London University, which has in the past done so much for medical education and examinations, whatever it may do in the future. Nor, I hope, will it be out of place to express satisfaction that more liberal ideas have at length prevailed in the older English Universities, that the barriers of exclusion which restricted admission to these Universities until within quite a recent period have been entirely broken down, and that they now open their doors to all comers, while joining heartily in the promotion of medical education and science.

Of all the agencies which have contributed to the advance of medicine, none has been more conspicuous or potent than the general recognition of the essential value and far-reaching importance of original investigation and research in a variety of directions, especially by experimental methods, and the ever-widening application of this principle as new fields of inquiry have opened up. It is impossible to form any adequate estimate of what medical science and art owe to the adoption and progressive working out of this great principle, which takes nothing for granted, but aims at the establishment of knowledge on the basis of facts, while at the same time constantly seeking for fresh discoveries. Beginning on a most limited scale, and with very inadequate instruments or means, the system has advanced in an extraordinary degree, and we see the outcome in the magnificent and thoroughly equipped laboratories of all kinds which to-day form a recognised and integral part of our Scientific Institutions, Medical Schools, and Universities. Moreover, while a few years ago this kind of work was carried on only by the few, the spirit of research seems to have taken such firm hold upon the rising

generations of the medical profession that the number of original workers in research laboratories and elsewhere is becoming almost embarrassing. And let me here remark that it is not only in laboratories that investigation and research may be carried on. Especially must we never forget to pay due homage to the splendid work achieved by the great clinical observers of this century, who have done so much to advance the science and practice of medicine and surgery. Before leaving this topic, although I hope and believe there can be no difference of opinion on the point amongst those present here this evening, I would ask, where would medicine have been to-day if it had not been for vivisection, by which alone has it been rendered possible to make a large number of discoveries and to establish many truths of inestimable value in relation to physiology, pathology, pharmacology, and other branches of knowledge, upon the results of which so many practical advances have been founded in relation to the healing art?

Such are the main factors which in different ways and degrees have, as it seems to me, contributed to the progress of medicine during the present century. They have been materially helped by the remarkable increase in the number and variety of Hospitals and allied Institutions, and their adaptation for clinical instruction and research; by the extraordinary development of medical literature of all kinds, which to-day has become, indeed, almost overwhelming in its amount and diversity; and by the opportunities and encouragement afforded for bringing before the profession the results of personal observation, experience, and investigation, and for the discussion of current topics of interest or importance, by this and kindred Societies, to say nothing of the larger Associations, and in more recent times of Congresses of all kinds. I much regret that time will not permit me to allude further to these subjects, full of practical interest though they be.

What, then, have been the practical results upon the medical profession arising from the changes which I have just outlined? and on what grounds do we rest our claim that medicine has made such material progress during the nineteenth century?

1. It must be evident to the most cursory observer who has even a superficial knowledge of the facts, that the entire profession occupies an incomparably better and higher position to-day than it has ever done before, and that the general standard of

knowledge and skill has steadily risen even within recent years. I desire to speak with the greatest respect of the practitioners of former generations, many of whom, perhaps, knew more than we give them credit for, or if they did not they seem to have made a very good show of "cleverness," and to have impressed their patients by their general "get-up," and what we would to-day call "putting on side," in a way that few of us can emulate at the present time. But, obviously, at the beginning of the century even the best of them could have had but a very limited acquaintance with the human body, either in health or disease, or how to deal with it; while the bulk of practitioners must have been in a state of profound ignorance and stagnation, and entirely guided by the teachings of tradition and authority. Gradually the profession was roused from this condition of apathy, and by the middle of the century had made considerable progress, as I can testify from personal knowledge. At that period a large proportion of the general practitioners, not to speak of consulting physicians and surgeons, might be described as fairly competent and skilful according to their lights, and many began to take an active interest in their vocation. From that time things have steadily and of late rapidly improved, and I unhesitatingly affirm that the average standard of knowledge and skill amongst the members of the medical profession is to-day remarkably high; while the great majority take a keen and intelligent interest in the march of events, exhibiting an earnest desire to keep abreast of the times, and to take advantage of every new discovery which can be utilised for the benefit of their patients, whether in diagnosis or treatment. Moreover, an ever-increasing proportion of the coming generations, when they finish their career as students, or even before that time, are so crammed with knowledge and science, so up-to-date in the most recent discoveries, inventions, and theories, and so really skilful, that they are a terror to their unfortunate teachers, especially those who have reached the stage of the "obsolescent senior," when age is beginning to tell upon their mental faculties; can even easily embarrass an examiner; and are equal to any position or emergency. Hence an increasing difficulty is being experienced in finding suitable spheres of work for these gentlemen, each of whom naturally, and no doubt rightly, regards himself as pre-eminently qualified to occupy any important post which happens

to become vacant, and thus there arises such a keen competition for hospital, teaching, and other appointments, of which unfortunately the available number is quite inadequate to provide for the many highly trained and accomplished candidates who apply for them. At the same time the ranks of the "consultants," and of the numerous "specialists," are unquestionably becoming uncomfortably overcrowded.

2. I presume that even those who are least disposed to say a good word for the medical profession must recognise the fact that our knowledge regarding morbid processes and conditions generally, as well as individual diseases and symptoms, has materially advanced during the present century, and especially during its later years. To us, however, who are more definitely acquainted with the existing state of things, and can compare them intelligently with the past, the changes in this respect must seem prodigious, and we cannot but feel amazed when we try to realise what has actually been accomplished. With respect to pathological conditions and processes, many of them, with which we are now quite familiar and thoroughly understand, in the earlier part of the century were either entirely unknown or the most vague and absurd notions and theories were entertained about them; and while others are still the subjects of investigation and controversy, we at any rate know a good deal concerning their nature, and may fairly anticipate that before long any doubtful or obscure questions relating to them will be cleared up. Moreover, we have gained a far more intelligent and accurate conception of, and insight into, the nature and the pathological and clinical history of many diseases long recognised in medical nomenclature; complaints which were formerly included under a particular name have been shown to be distinct, and are now duly discriminated and classified; while affections which must have stared the profession in the face from time immemorial have only been definitely made out within a comparatively recent period, though some of the more novel diseases which are brought under our notice seem really to be developments of the nineteenth century. I dare not begin to illustrate these statements, but examples will readily occur to your minds. I merely offer them as a feeble expression of what medicine has done in the way of dispelling ignorance and establishing knowledge on a true foundation during the period which we are now discussing.

3. One of the greatest benefits which the nineteenth century has conferred upon humanity in relation to medicine is the flood of light which it has thrown upon the vast field of etiology, and the striking practical results which have been worked out in connection with this branch. The whole subject has been opened up and studied on broad and intelligent lines, as well as with remarkable attention to detail and painstaking investigation, with the result that not only has the causation of disease been made clear from its general aspects, but the relation between particular causes and a large number of the most important complaints with which we have to contend in the course of practice has been definitely determined. In this connection it would obviously be impossible to avoid mentioning the living organisms which are now so generally recognised as etiological factors, and which in modern times have worked such a profound and far-reaching change in our views and practice with regard to so many grave diseases. The progress made in our knowledge of etiological facts, general and special, and their practical application, has not only enabled us as a profession to carry out preventive treatment in numberless individual cases on rational lines and with the greatest success, but has laid the foundation for that most important department, now so popular, which is concerned with Hygiene, Sanitary Science, and Public Health. This department not only aims at the prevention of disease on an extensive scale, but also at the extermination or, at any rate, the practical control of some of the most grave and deadly maladies which afflict humanity, as well as the promotion and maintenance of sound health in the community at large. Truly a memorable and noble purpose and aim, to which the nineteenth century has practically given birth !

4. Another striking and most helpful advance for which the present century will ever be memorable is the remarkable addition to, and development of, the methods of clinical investigation applicable for a variety of purposes, many of which we now regard as so essential for arriving at a positive and correct diagnosis. Most of these methods, moreover, are well within the scope and capacity of any properly trained practitioner, and are supposed and expected to be employed habitually and as a matter of routine. But even those which require the help of a specially skilled expert or which call for more or less elaborate scientific

investigations, chemical or other, are now fully at our service. What a contrast to the condition of things 100 years ago! Imagine a time when there was not even a wooden stethoscope, far less the more or less elaborate and artistic binaural instrument now generally employed, or other more scientific kinds of auscultatory apparatus; when the clinical thermometer had not been invented; when systematic examination of the urine was unknown as a means of diagnosis; and when there was no such thing as a microscope at the command of the busy practitioner! The progressive development of clinical methods is a highly interesting study, culminating in that most startling revelation or outcome of modern science, "Skiagraphy," which promises to supplant all other methods, and to enable us actually to see what is going on within every part of the human body without taking any further trouble. In the meantime, however, may I venture to suggest that it may be well for us not to rely too implicitly on these aids to diagnosis, lest we become mere machines, and lose altogether the faculty of using our external senses in a simple way after the fashion of our ancestors, as well as the ability to exercise and utilise the mental powers, to which they must have owed the keen clinical insight with which many of the practitioners of past generations were undoubtedly gifted.

5. When we call to mind that the ostensible purpose for which the medical profession exists, at any rate from the point of view of the laity, is the treatment and especially the cure of the various maladies which afflict humanity, it is not to be wondered at if its progress during the past century should be gauged in the main by its achievements in this direction. We may be fairly asked what real and lasting advance has been made in scientific and practical therapeutics and in the "healing art"? There have been many and many changes in treatment, and vaunted "cures" without end which were going to work wonders; but how far have they held their ground, and proved of positive service and reliable, instead of mere will-o'-the-wisps? In answering these questions I will venture to trouble you with a brief sketch of the course of events in relation to therapeutics which has occurred within my own memory and experience, but before doing so I may affirm without hesitation that this century has in large measure revolutionised our ideas about the principles and methods of treatment, has emancipated us from narrow and erroneous views, and, not

forgetting all its pretensions and failures, has added to our weapons for combating disease to an extent which can hardly be realised except by those who have actually watched the progress of the times, and can, from their own knowledge and observation, compare the present with the past.

During the earlier part of the century treatment was to all intents and purposes a matter of unquestioned tradition, handed down from generation to generation—"echoes from echoes," as I have seen it described—and of rigorous and undeviating routine. The so-called "antiphlogistic" system held special and predominant sway, and beyond that there was but a very narrow and limited range of therapeutic agents or remedies to choose from, and these consisted entirely of drugs. Here and there some independent spirit would assert himself and start some novel treatment, but he got few, if any, followers, and his method soon died a natural death. This was practically the state of things which I remember as an apprentice, though I am aware that at that very time there were in the more active centres of professional life signs of awakening from the dull lethargy of tradition and routine, and of the breaking down of the formidable barriers to progress which they had raised. I look back to that time with somewhat mixed feelings. I will say this, that apart from the antiphlogistic treatment to which I have already referred, there was, at any rate, little or no harm done by the medicines which I had to dispense in an ordinary way. As a rule, they were of the most simple and inoffensive character, and should one of the very few strong drugs which we employed be prescribed, it was in such small doses that it could injure nobody. As regards antiphlogistic treatment, however, I shudder when I think of it. It was put into force on the smallest provocation, and any poor patient who presented the slightest suspicion of fever or an inflammatory disease, had a very bad time as a rule. Bleeding as an independent treatment was also rather extensively practised for various conditions, and often for no valid reason. But what I look back upon with most concern are epidemics, especially of smallpox, cholera, and what was then called "febricula," "synocha," or "continued fever," but which we now know to have been typhoid fever, which were treated on the most approved principles then in fashion, with the most lamentable results as regards mortality, though I will say that

these were due rather to faults of omission than of commission. I think this is a fair example of the average state of things about the middle of the century.

When a few leading members of the profession were roused from the general apathy, and began to observe and think and act for themselves, which was the first step towards quitting the beaten track, the earliest and most striking result which I can call to mind was the revolt against the antiphlogistic system as applied to febrile and inflammatory diseases, the substitution of the supporting system for that of depletion and starvation, and the special advocacy of the stimulant method of treatment by Todd, Bennett, and others, which soon became very popular and, as the natural result of undue reaction, was, of course, carried a great deal too far. After this came the time when the effects of therapeutic agents upon the human body and upon its several organs and systems, both in health and disease, began to be observed and studied methodically and scientifically. The next step was the application of the experimental method to living animals, by which the effects of a large number of agents have been investigated systematically and thoroughly, to the incalculable advantage of practical therapeutics, anti-vivisectionists notwithstanding. This line of research is now in full swing in a large number of centres in this and other countries, and the establishment of special pharmacological laboratories testifies to the firm hold which it has taken upon the profession. At the same time the clinical study of the action and uses of remedies has progressed steadily and uninterruptedly, on an ever-widening scale, and by an increasing number of observers, with the most satisfactory and beneficial results in the way of testing agents, new and old, as to their real value and worth, and helping to establish scientific and practical therapeutics on a rational and reliable basis.

I have already referred to preventive treatment, and now propose to take a comprehensive survey of the outcome of the progressive development of the study of pharmacology and therapeutics which I have thus briefly noticed, and to endeavour to form some estimate of what has actually been done in the way of advancing and improving the practical treatment of disease. Standing out conspicuously and pre-eminently above all others two achievements of this century must be ever regarded with

feelings of deep gratitude, wonder, and admiration: (1) the introduction and development of anæsthetics, general and local, which have been of such incalculable service in the prevention and relief of pain as well as in other directions, and the possibilities of which seem even yet not to have been fully realised or exhausted; and (2) the working out on scientific lines, as well as on a rational and intelligent practical basis, of the great principle of antiseptic and aseptic treatment, which is now so universally acknowledged and adopted in many directions and for a variety of purposes. Whatever modifications of the views originally held and the practice founded thereon which further experience may have brought about, nothing can detract from the honour due to Lister for his patient investigations and splendid work in this department. Dr. Brouardel, of Paris, when recently paying a tribute of admiration and regard to this eminent scientist and surgeon, affirmed that up to the present time at least 200,000 lives had been saved by his discovery, and probably even this estimate is far short of the mark.

Another result which the medical profession may congratulate itself upon without question is the addition to our available remedies of many genuinely useful and efficient new drugs, as well as of excellent and valuable pharmaceutical preparations and combinations, by no means limited to those officially recognised in the British Pharmacopœia. Of course, for many of these we are indebted to the remarkable progress made in scientific and practical pharmacy. We cannot boast of many "cures" for particular diseases, and have to be content, in the main, with those known to our ancestors for many a day, for the simple reason that the complaints which can be thus directly influenced are, so far as our present knowledge goes, and probably from their very nature, comparatively few. Of course, I am speaking of what I imagine to be the general experience, and do not presume to answer for those heaven-born geniuses who have succeeded in discovering an unfailing remedy for every individual disease; who can invariably "cut short" the most grave and dangerous maladies, or make them run the mildest possible course; and who never lose a case of any description! Taking a fair and reasonable view of the matter, what I think we can claim as a profession is that we have to-day at our command a far larger and increasing number of really useful medicinal agents than in past times, and

that we understand better how to employ both the old and the new, and for what purposes. We are now decidedly more able than formerly by our remedies to influence and modify morbid processes and conditions, to affect important functions, and to cure or relieve symptoms of various kinds which constitute such a large item of the service we are expected to render in ordinary practice. On the other hand, it must be confessed that out of the enormous number of medicinal agents brought under our notice by puffing advertisements in the Press, medical as well as lay, by pamphlets or even large books delivered by post, or by actual "specimens for trial" which are nowadays so liberally delivered at our residences, comparatively few hold their ground, or stand a fair and candid criticism and investigation of their vaunted merits. Still a certain proportion do, and I see every reason to anticipate that, as the result of the systematic researches, scientific and practical, now carried on in so many laboratories valuable additions will be made from time to time to the medicinal agents at our disposal for the help and comfort of our patients. I only hope that in our love for the new we will not entirely throw over old friends, which have done real and effective service in the past, and are to-day as deserving of our regard as ever.

A distinct advance in practical therapeutics which must not be forgotten as a development of this century is in the modes of administration of therapeutic agents; the preparations and appliances intended for local purposes; and the instruments or apparatus invented for making applications to, or otherwise treating, special parts. At the present day a visit to the exhibition usually arranged for our edification at the annual meeting of the British Medical Association, and at various Congresses, is a revelation as to what has been and is being accomplished in this direction, in which, of course, our profession is greatly aided by pharmacists and instrument-makers. I will merely refer to the many palatable, convenient, compact, and elegant preparations given to us by pharmaceutical chemists for internal administration, in contrast with the boluses, powders, electuaries, and horribly nauseous draughts and mixtures of the old pharmacy; and to the introduction of the hypodermic syringe, the value of which to the practitioner can hardly be over-estimated. Undoubtedly there are serious drawbacks associated with these

advances which we must never ignore, and these are the facility with which the laity can now get strong drugs in convenient forms for their own use without any medical supervision, and the danger of falling into the habit of using powerful subcutaneous injections, which has done, and is still doing, such serious mischief, not only amongst patients but also to members of our own profession, a fact which is but too familiar.

During the earlier part of this century, and indeed until within a comparatively recent period, treatment consisted almost entirely of the use of drugs in different ways. By degrees the great principles of hygienic treatment took hold of the profession and the public, and they are now well to the fore and highly popular, at any rate in theory. Surely this is a great step in advance in practical therapeutics, and nowadays, apart from the more obvious general hygienic measures, the means for carrying out some of the most important of these principles, such as change of air, suitable climate, sea voyages, the use of baths, and the like, are so increased and improved, and so much within the reach of large numbers of the community, even to some extent among the poorer classes, that there is no excuse for neglecting them. Diet, too, has come to occupy a far more conspicuous place in treatment than formerly, and is regulated on more scientific principles, certainly to the general advantage, though there is, perhaps, a tendency in the profession on the whole to make more of questions relating to food and drink than seems absolutely necessary, which renders it very difficult to deal with not a few patients who, being quite "up-to-date," expect to receive the most explicit directions as to diet under every conceivable circumstance, and suitable for every complaint, real or supposed, from which they may happen to suffer. And what shall be said about nursing, which to-day is so universally and rightly regarded as one of the most important and essential parts of modern treatment? Some of us here present must have a vivid recollection of the hospital nurse of former days, who was as incompetent as she was unattractive and insanitary, and of the time when private nursing as it now exists was practically unknown. The progress that has been made in this department is simply amazing, and those of you who are only familiar with the present state of things can have no possible conception of the magnitude of the changes which have taken place during the last 30 or

40 years, and of the comparative advantages in the way of capable and reliable nursing which modern treatment affords.

A conspicuous, and within due limits really serviceable, development of modern therapeutics is the introduction or more systematic working-out of what I may term "special methods" or "combinations of methods," which are still being added to, and intended either for the purpose of producing some particular effect which might be beneficial in various conditions, or for the treatment of some individual disease or class of diseases. It would be quite out of place to attempt even to enumerate these methods on the present occasion, and I will merely remark that they are not all really novel, some being the revival of methods formerly practised, or the application, more or less elaborate, of well-known physical principles. Others, however, are the natural outcome of the more or less modern discovery of forces, or elements, or facts previously unknown, which under judicious and intelligent guidance have been found to be highly valuable in treatment when properly employed. Others, again, are essentially recent and original, especially those which we owe to the truly astonishing and far-reaching results of investigation and research in relation to bacteriology and kindred subjects. Taking a comprehensive view of these modern special methods, they certainly are so far to be commended that they present a practical protest against the needless and excessive use of drugs, and are, when rationally and honestly employed, often of unquestionable advantage in treatment. Some of them give evidence of the possession, on the part of those who are responsible for their introduction to the profession or their practical working, of considerable ingenuity and inventive ability, others of a profound knowledge of human nature. I am sorry to say that they afford excellent opportunities for the most outrageous quacks outside the profession to enrich themselves at the expense of the idiotic simpletons who are still found in such large numbers even in this highly educated and enlightened age. Of this statement I could give some glaring and really ludicrous examples from personal knowledge. But what can we expect when so many men and women of intellect and culture, aggressively scientific, will to-day throw over everything that is orthodox in our profession, and pin their faith upon any system, however pretentious and absurd

it may be, which gives itself a high-sounding name, and will even prostrate themselves before the Christian Scientist!

It would ill become me as a physician if I did not on the present occasion, in relation to the progress in treatment which I am now discussing, pay my tribute of respect and admiration to modern surgery, which has achieved such marvellous and brilliant results in a variety of directions. I well remember the time, and have myself witnessed the performance, when the highest boast of the operating surgeon was that he could amputate a limb, remove a breast, or cut out a stone within so many seconds, with little or no regard or care for the ultimate issue of the case. At one time I had a good deal to do with surgery, and my recollections of its operative aspect and of surgical treatment generally in those days, negative as well as positive, are anything but happy or pleasant. Speaking to-night as a physician, I feel sure I am expressing the sentiments of my colleagues, as well as of practitioners generally, when I gratefully acknowledge our indebtedness for the improvements in minor surgery which have proved of such value in the treatment of medical cases, when we have to deal with internal accumulations of fluid and various other morbid conditions. But this advance sinks almost into insignificance in comparison with the astounding achievements of surgeons as regards the formidable operations which they now perform in connection with the internal cavities, especially the abdomen, as a matter of course, and with such remarkable success. I can myself call to mind but too vividly not a few cases in former days, particularly of thoracic and abdominal disease, in which the patients were allowed to die unrelieved, or to drift hopelessly through much suffering to an inevitably fatal termination, where to-day surgery would step in, and almost to a certainty avert death at a critical time, or ultimately save life and restore the sufferer to health and vigour. Of course, these achievements could not have been possible were it not for the invaluable aid afforded by anæsthetics and antiseptic or aseptic treatment. These, however, only gave the opportunity for the manifestation and exercise of latent and dormant capacities and powers in individuals, which have gone on steadily developing and improving until they have brought us to the present stage almost of perfection, though the extreme limits of operative treatment seem to have been by no means yet reached. Personally, I regard it as an honour to belong to

a profession which can boast of so many members whose originality in conceiving, devising, and working out the most formidable operations: whose manual skill and dexterity: whose confidence, nerve, daring, and pluck: whose striking and devoted attention to detail: and whose intelligent and painstaking after-treatment have conferred such inestimable benefits upon suffering humanity, and at the same time have made the nineteenth century memorable for the splendid position to which they have raised the Art of Surgery.

I have thus endeavoured, however imperfectly, to give some comprehensive sketch of what strike me as the most prominent and important changes which have guided the course of the medical profession during the century which is now rapidly drawing to its close, and the lines upon which progress has taken place. I by no means wish to make ourselves out better than we are, and we must all recognise the fact, but too evident, that there are many dangers and difficulties and drawbacks in connection with our vocation, both in its internal workings and its external relationships, which cannot fail to rouse feelings of anxiety and concern as to the future in any thoughtful mind. But let us to-night be optimistic, and take a bright and hopeful view of things. The Archbishop of Canterbury recently expressed his belief "that there could not easily be found, all the world over, a body of men who approached more nearly to the full discharge of their duty than the clergy of the Church of England." That sentiment I unhesitatingly appropriate for the great bulk of the Medical Profession. And looking back upon the nineteenth century I venture to affirm, with a feeling of assurance and pride, that their efforts during this period have contributed in a degree which can never be adequately realised or estimated to the saving and prolongation of life, to the cure and prevention of disease, to the eradication or great amelioration of many grave and formidable maladies, to the practical relief of human suffering, and to the promotion of the health and general well-being of the community at large.

DISCUSSION ON "SERUM THERAPEUTICS AND ANTI-TOXIN TREATMENT."

Dr. Heron said that he had been informed by the President that each speaker in this discussion would be required to confine his remarks within a time limit of ten minutes, and had been invited by him to do what he could, within that time limit, to touch in the most general terms upon the results tuberculin had given when used in his hospital wards in the treatment of tuberculosis of the skin and of the lungs.

The fourteenth volume of the 'Transactions' of this Society contains the record of what he then had to say concerning the treatment of lupus and of tuberculosis of the lungs with tuberculin. It was on April 20th, 1891, that he read to this Society the paper detailing his then experience of this method of treatment. In that paper the cases of 37 patients were discussed at some length; 32 of these were cases of tuberculosis of the lung, and five of them were cases of lupus. From no important statement in that paper had he now anything to withdraw.

The cases of lupus all improved up to a certain point. The improvement was very considerable; but all the five relapsed, one of them not till a year after leaving hospital. Two of them relapsed during treatment, while they were daily receiving doses of from 200 mlgr. to 1,000 mlgr. of the old tuberculin.

Concerning the outcome of the treatment of the 32 cases of tuberculosis of the lung, which he had treated with the old tuberculin in hospital in 1891, the following was the latest information he had:—Eight of them were dead; one worked as a domestic servant, and remained well until the autumn of 1897, when she began to get thin, and eventually showed unmistakable signs of a return of pulmonary tuberculosis; 10 of these patients were now fairly well; and of the remaining 13 cases he could find no trace—a fact which will surprise no one who knows how very difficult it is to keep in view patients who have left a London hospital. This information concerning these 37 patients brought his knowledge of them up to June, 1898.

In June, 1898, in a post-graduate lecture reported in the 'British Medical Journal' of July 9th, 1898, he had referred to nine cases of tuberculosis of the lung and one of lupus which he had treated in hospital with Koch's new tuberculin. Of these nine people two were dead. In the published lecture it will be found recorded that they were both, when he first saw them, practically hopeless cases; and one of them, for reasons which will be found stated in the lecture, was selected for the treatment because he was certainly slowly dying. He died, having received 26 injections of the remedy between April 14th and May 26th, 1897. *Post mortem* it was found by his colleague, Dr. Perkins, then pathologist to the hospital, that there was no trace of recent tuberculosis in the viscera. This fact is important, because it has been stated by Professor Virchow that the use of the old tuberculin is apt to be followed by excavation of the diseased portions of the lung, and the consequent carrying of infection by means of the morbid material taken up by the circulation at those points of excavation, and conveyed by it to healthy portions of the body. In this case of his no evidence of any such spread

of infection was found. He would here add that in no case in his own practice had he seen evidence of harm having come to a case of tuberculosis in consequence of its having been treated with the new or the old tuberculin. Over 2,000 injections of tuberculin had been given in his hospital wards, and in no case had there been even a superficial abscess at the site of puncture.

To the other case of death among the 10 patients now referred to he need not further allude. It was recorded in the 'British Medical Journal's' report of his lecture. The remaining seven cases of tuberculosis of the lung all did very well in hospital. Without exception they left the hospital at their own request, stating that they felt well enough to return to work, and wished to do so. They all improved in health to a remarkable degree. Last summer (1899) four of these seven cases were at work and in good health; of two cases he had lost trace, and one continued well until about the end of 1897, when he heard the patient was out of work and destitute, but he was not found or heard of after that time.

It will be remembered that in this batch of cases one of lupus was included. It is referred to at considerable length in the lecture already mentioned, and, as there was now no time for detail, he would only say of that case that 18 months after leaving hospital the patient had suffered no relapse, and the scar tissue then remained healthy.

There had been under treatment in his wards during this year 10 cases of tuberculosis of the lungs. In one of these, a woman, the treatment was discontinued. She had, before tuberculin was given to her, a high temperature. The temperature remained high, although a few very small doses of tuberculin were given. They had, in this case, to deal not only with tuberculosis of the lungs, but with the results of a mixed infection of those organs. Another of these 10 cases, a man, came under the same class of patient as the case just mentioned, and for the same reason the use of the remedy was, in his case, also discontinued. Of the remaining eight cases, three women are still in hospital under treatment and improving in health. Another woman comes to hospital and receives a weekly injection of the contents of one bottleful of tuberculin. She has greatly improved in health. Four men also visit the hospital at stated intervals and receive injections of large doses of tuberculin. They have all improved and are still improving in health.

For this meagre account of his experiences of the use of tuberculin he craved the kind indulgence of the President and of the Society, as in the time allotted he could say no more. He proposed, by-and-bye, to state at some length the details of the last 10 cases to which he had just now referred.

In conclusion, Dr. Heron wished to point out that Koch himself had said that the case most likely to derive the greatest benefit from the use of the new tuberculin was one that had rarely, or, better still, never, a temperature rising above 99·4°, and in which only a small portion of one lung was affected. Not one of the 10 cases last mentioned came within this category, and, in his experience, such cases were far from common among hospital patients in London. From what he had seen of the literature of this subject, he was sure what Koch had said about the typically hopeful case was too often forgotten.

Dr. Washbourn said he should confine his remarks to the anti-diphtheritic serum, the anti-streptococcic serum, and the anti-pneumococcic serum, because he had had most experience with these sera.

He called attention to the fact that there were two distinct classes of curative sera, according as they were anti-toxic or anti-bacterial. The first, such for example as the diphtheria serum, annulled the effects of the bacterial toxins. They only had an indirect effect on the bacteria by annulling the injurious effects of the toxins. The other class, such as the anti-streptococcic and anti-pneumococcic sera, produced their effects by preventing the growth of the living bacteria in the body, but they had no effect whatever in annulling the toxins. With respect to diphtheria anti-toxin, it had unquestionably greatly contributed to diminish the case mortality from diphtheria. An immense amount of statistics from all parts of the world went to prove this, as did also the statistics of the Metropolitan Asylums Board and the cases which had been analysed by a Committee of the Clinical Society.

It was very important to understand the way in which diphtheria anti-toxin acted. He himself adhered to the theory of Ehrlich, who held the view that all toxins, and probably all poisons, produced their injurious effects in virtue of their power of combining with certain molecules in the living cells of the tissues. If the substance had no such power it was harmless. That constituted the whole difference between a susceptible and an insusceptible animal ; the cells of the one possessed these molecules, while the other was free from them. He assumed that the diphtheria anti-toxin had the power of combining with the toxin, and so prevented it from combining with the molecules in the cells. It might be asked, he observed, how it was that diphtheria anti-toxin did not save all cases which, theoretically, it ought to do. The deaths which occurred in spite of treatment might be explained by the fact that the anti-toxins which one injected did not always possess a sufficient affinity for the toxin to prevent the latter acting upon the cells. In other words that there were certain tissues of the human body which possessed a greater affinity for the toxins than the anti-toxin did, viz., the tissues of the nervous system. The vast majority of the patients who died under treatment exhibited certain definite symptoms. After anti-toxin had been injected the patient got better, the membrane cleared off, and one imagined that he was going to get well. Then the heart got rapid and irregular, vomiting and perhaps abdominal pain supervened—symptoms probably due to paralysis of the vagus. He believed that, although the anti-toxin had combined with the toxin, the affinity of the nervous system was so great that it was able to extract the toxin from this combination. If this were the true explanation, they might hope that in the future it would be possible to produce an anti-toxin which would display a greater affinity for the toxin than did the nervous system. He suggested that it might be possible to prepare anti-toxin from other animals than those at present employed for the purpose, anti-toxins which would enable them to prevent this class of complication.

Discussing the question whether anti-toxin should be administered in every case of diphtheria, he observed that if it were an absolutely harmless substance, the reply no doubt would be in the affirmative ; but it was not an absolutely harmless substance. Even a fatal result could not always be avoided in spite of every precaution. There were dangers of the serum being contaminated, of which he had seen two examples. He maintained, therefore, that anti-toxin should not be administered in every case. The class of cases in which it was undesirable was that in which the patient was first seen four or five days after the disease had

begun, without constitutional symptoms having developed. Such cases did not require it. He would not give it either in cases where there was serious cardiac affection arising from the diphtheria. He thought, too, that very mild cases did not require this treatment, but he admitted that this decision required considerable judgment, seeing that apparently mild cases sometimes developed into severe ones. Nevertheless, it was a good general rule that one should administer anti-toxin in every case in the absence of some special reason to the contrary.

With regard to anti-streptococcic serum the points of importance were these—that no specimen of anti-streptococcic serum would protect against every kind of streptococcus, a point that had been well borne out by clinical experience in the use of this serum. He had found that in some cases it had not the slightest influence, while in others it produced a very marked effect. He referred in particular to a case of infective endocarditis, in which he believed he had saved the patient's life by the aid of this serum. The patient had a high temperature with marked septic symptoms, a bruit over the pulmonary area, and was rapidly going to the bad. She began to recover after the commencement of the treatment, and in four or five weeks got quite well, except that the mechanical injury to the valve remained. He had also seen cases of erysipelas in which the patients were getting worse, but began to recover as soon as the injections were commenced. To sum up, the serum had a most remarkable effect upon certain cases of streptococcic infection and none whatever in others, and unfortunately there were, at present, no means of distinguishing between these cases.

As to the anti-pneumococcic serum, in which he was specially interested as having been the first to prepare it on a large scale, it had very definite protective powers when tested upon animals. Pane, of Naples, now prepares it on a much larger scale, so that it could easily be obtained for clinical use. Pane's serum was prepared in the same way as his own. He had on several occasions tested this serum upon animals in conjunction with Dr. Eyre. A cubic centimetre will protect against a thousand fatal doses of the active microbe. In Italy the serum had been used very extensively, and good reports of its efficiency had been given. In his own experience beneficial effects certainly appeared to have been obtained in some cases. The protective power of the serum was, however, based upon such good experimental proofs that it was worthy of more extended trial in this country. The dose of it was the same as with anti-streptococcic serum.

The general results of his own experience were that there could be no doubt whatever as to the beneficial effects of the diphtheria anti-toxin; that the anti-streptococcic serum was useful in certain cases; but that in respect of the anti-pneumococcic serum he would not hazard a definite opinion until he had had more experience of its use.

Dr. Hubbard did not propose to touch upon the anti-toxic power of serum, but would confine his remarks to its coagulating properties. Animal serum, as was well known, had the power of causing coagulation in hydrocele fluid and in other coagulable fluids. He had made use of this property by giving various serums in aneurysms, in menorrhagia, and in hæmophilia. In March, 1896, a patient came to him with aneurysm of the aorta. The diagnosis was perfectly obvious, the tumour being very large and the pulsation very marked. At that time (and this illustrated the fact that, in respect of its coagulating property, one serum

was as good as another) there was being sold an anti-syphilitic serum, and, as he thought that this patient might have suffered from syphilis, it occurred to him to attack the possible syphilitic factor. He therefore injected the serum, repeating the dose on the two following days. Before the injections the cardinal symptom had been pain of intolerable intensity. By the third day the pain had disappeared, though the patient had continued to get about and no other treatment had been employed. She continued her ordinary life until October of that year, without any recurrence of the pain. She came back on that date, complaining of the return of the pain, and asking for further injections. Knowing that it could not be any anti-syphilitic power that had acted in this manner, and judging that the effect must have been due to the coagulating power of the serum, he then made use of anti-diphtheritic serum, and the pain disappeared within 24 hours. It recurred a week or two later, but not severely, and then for the first time he put her on starvation diet and iodide, with the result that the coagulability came again into play, and the pain disappeared. She remained well for a long time, but he believed that the case ultimately went to the bad. He also had another case of aneurysm in which good results followed the injections, but in which the aortic valves were too seriously involved to do very much good.

With regard to menorrhagia, he had had one or two cases in which the coagulating power of the serums had a very marked effect, particularly one in the autumn of last year. This was a severe case in a single young woman, without obvious pelvic lesion. He had heard from the patient this summer, and there had been no recurrence.

As to hæmophilia, Mr. Wallis had seen a case with him. The patient was a young man who had profuse bleeding from the nose, which had been plugged by various medical men, but on removal of the plug the bleeding recurred, and the patient nearly died from the hæmorrhage. At this juncture the patient came under his care, and he asked Mr. Wallis to see the case with him. Mr. Wallis applied the electro-cautery. The patient left the Nursing Home much too soon, and it fell to him to remove the plug, but directly he began to do so the bleeding recommenced in the most alarming way, so he replaced the plug and sent for Mr. Wallis. They determined to try the effect of serum, and gave an injection of anti-streptococcic serum, followed the next day by a dose of anti-diphtheritic serum. On the day following the last injection he removed the plug without the loss of more than 10 drops of blood. He thought that this was a fresh departure, for he had not heard of the coagulating power of serum being made use of therapeutically. He thought it opened up a very interesting department of investigation, which might prove of considerable value.

There remained the interesting question whether normal serum was as powerful as the anti-toxic as a coagulating agent. He had carried out experiments with a view to determine that question, but so far without any definite result. He had, however, some reason to believe that the anti-diphtheritic serum was the most active coagulant.

October 23rd, 1899.

THE INFECTIVITY OF MALIGNANT GROWTHS.

By G. Bellingham Smith, M.B., B.S., F.R.C.S., and J. W. Washbourn, M.D., F.R.C.P.

Theories Regarding the Etiology of New Growths.

In discussing the infectivity of malignant growths, we shall first consider very shortly some of the theories that have been brought forward to explain the causation of new growths. Such theories as that which ascribes tumours to some influence exerted by the nervous system, or that which regards tumours as idiopathic in origin, we may dismiss at once. The theories which regard the influence exerted by heredity, or which look upon a certain dyscrasia or fitness of soil as sufficient to explain the phenomena of new growths, need not detain us. That they are factors of importance in regard to predisposition, there can be no doubt; but as they are not capable of explaining either why a growth should make its appearance, or the phenomena of malignancy, we shall not discuss them any further.

The *embryonal theory* of Cohnheim undoubtedly offers a satisfactory explanation of some tumours. According to this view, new growths originate in persistent remnants of embryonic tissue that have not been used up in the course of development. And the possibility of tumours developing from these remnants is explained by the inherent power of growth possessed by embryonic tissue. As examples of tumours the origin of which can be satisfactorily explained by this theory, we may mention the enchondromata of bone and the congenital sacro-coccygeal tumours. This theory further receives some confirmation from the results obtained by transplantation of fœtal structures to adult tissues. Such grafts have been shown to be capable of a certain amount of growth, though always to a limited extent.* But as a satisfactory explanation of the etiology and phenomena of all growths, more especially the malignant growths, and their infective characteristics, Cohnheim's theory fails.

* On this subject we would refer to a recent paper by Birch-Hirschfeld and Garten in 'Beitr. z. path. Anat. u. z. allg. Path.,' Jena, Bd. xxvi, Heft 1.

The next theory we shall consider is the *inflammatory or irritation theory*. The lines of distinction between inflammatory hyperplasias and new growths are by no means sharply defined, more especially as regards tumours of the connective tissue type. Shattock has put on record the case of a large fibroma occurring in the pelvis, which contained in its centre a piece of steel, which was evidently the essential cause of its formation. The influence of chronic inflammatory and irritative conditions on epithelium is well known. In tuberculous disease of the tongue, for instance, the overgrowth is occasionally so well marked that it has been taken, under the microscope, for epithelioma; and in chronic ulcers of the leg the epithelial proliferation is so pronounced that Formad some years ago considered that a large number of these ulcers were to be regarded as epitheliomatous. Malignant growths so frequently follow chronic inflammations and irritations, that we can readily understand how widely accepted is this theory of the causation of growths; and of its importance as a factor in their etiology perhaps too much stress cannot be laid, from a clinical point of view.

But whilst admitting these facts, and recognising that local irritation may give rise to local overgrowths, we are far from explaining the infective properties possessed by these local growths. To explain these we have to fall back on the theory that as the result of the epithelial proliferation certain cells get detached from their normal connections, and either as the result of inherent powers of indefinite multiplication, or from certain acquired powers, they are capable of giving rise to both local and distant deposits. This theory is practically a modification of that of Thiersch.

According to the views of this author, the explanation of carcinoma in later life is to be looked for rather in an alteration of the mutual relationships between the different tissues than in any increased activity on the part of the cells; that, in fact, there exists a natural antagonism between the epithelium and its surrounding connective tissue. In old age the connective tissue becomes looser in texture and its resistance lowered, and the epithelial cells proliferating, perhaps as the result of some local irritation, readily penetrate into the enlarged spaces and lymphatics. Here, in accordance with their life history, they continue to grow indefinitely.

According to this view, then, the epithelial cell detached from its normal connections may on occasion preserve all its original characters of continuous growth; and to this power of indefinite multiplication the phenomena of malignancy are ascribed. With, however, the exception of an experiment by Dr. Lambert Lack, we have no evidence, experimental or otherwise, to support this view. Transplanted tissues have never been shown to possess any power of indefinite growth. The skin graft having served the purpose for which it was intended, ceases growing. Its capacity for growth is limited by the necessities of the case. The nearest approach, perhaps, to new growths, resulting from epithelial transplantation, is seen in the implantation cysts, when a small portion of epithelium carried deep into the tissue beneath gives rise to a dermoid.

Lack's experiment may have a very important bearing, if not on the etiology of malignant tumours, at all events on their secondary infective characters. He opened the peritoneal cavity of a rabbit, and, having cut open an ovary, scraped the surface with a knife, allowing the milky juice thus obtained to diffuse through the peritoneal cavity. The rabbit remained well for nearly a year. It then became weaker, and suffered from dyspnœa, and two months later was killed. On *post-mortem* examination, masses of new growth were found scattered through the peritoneal cavity, pericardium, liver, and uterus. These masses on section showed the structure of a columnar-celled carcinoma.

This experiment must necessarily be repeated before the conditions found can be accepted as due to the operation performed. If we regard the experiment as it stands as satisfactory, we are led to the conclusion that a healthy epithelial cell, when detached from its normal connections and grafted elsewhere, is capable of indefinite multiplication, and of giving rise to a tumour which in its structure and behaviour presents all the characteristics of malignancy. And this takes us even further than Thiersch's view, for the latter presupposes some alteration in the relationships, owing to advancing age, between the epithelial and connective tissue structures.

With all our existing knowledge as regards transplantation of fœtal and adult tissues, this experiment is difficult to accept, but a still greater difficulty is met with when we try to explain some of the other features of malignant growths. We are led to

assume that the epithelial cell is capable of existence outside the body for variable periods of time, if we are to explain the infection that resides for a time in cancer houses and districts. Dr. Lack's experiment may, however, represent part of the truth in regard to the phenomena of malignancy, and for the present we must await its confirmation.

Returning now to the theory of irritation, we may look at the matter from another point of view, and consider whether in place of non-specific irritants we may not have some *definite specific cause of the disease*, that by its continued stimulus will account for the progressive growth, both locally and in distant parts, of the tumour. In this case such irritations as tooth-stumps, chronic ulceration, and the like, are to be regarded rather as a predisposing than as an actual exciting cause of malignant disease.

The question becomes a little clearer if, instead of taking a long-continued irritation as the suggested cause, we take one definite injury. The frequency with which a history of some injury is given as the cause of a sarcoma is so frequent that we cannot neglect it. We would refer especially to the valuable work of Dr. William Coley on the subject.

We may fairly ask why a blow or injury to a bone should in one case set up acute suppurative periostitis, in another chronic tuberculous disease, whilst in a third a sarcoma results. In the two former we now know that the blow is the predisposing cause only, the actual cause being a specific organism in each case. It is hard to resist the suggestion that in the third case we may also have as the essential factor some specific organism.

The analogy between tuberculosis and malignant growths is so well known, and has served so often as a basis for the view that these latter are due to some infective agent, that we need do little more than call attention to the fact. There is scarcely a feature in the one disease that has not its counterpart in the other. That cancer has, like tuberculosis, a local origin, is an accepted fact, for on it all our treatment of the disease at present depends. The mode of spread in both is similar, either by the lymphatics or by the blood stream. Even general miliary tuberculosis is simulated by cases of what are known as general sarcomatosis and carcinomatosis.

The points of resemblance have been well worked out by Shattock and Ballance. These authors point out how glandular

infection without obvious primary lesion is seen in both, and give as an example those cases of epithelioma in the inguinal glands of chimney sweeps, in which no primary growth is discovered. The latency of infection in the glands is also common to both. We have recently seen a case in which growth has made its appearance for the first time in the axillary glands three years after removal of the breast; and such cases in which the infective agent has apparently remained latent for years are by no means uncommon.

The appearance of a sarcoma, following a single definite injury, is on all fours with the similar appearance of a tuberculous lesion under the same circumstances. We have seen cancer of the tongue following an injury with a pipe stem, and in one case a skin carcinoma resulting very quickly from an injury which caused bruising and grazing of the cheek.

And, lastly, we have the cachexia. This condition is, as a rule, so closely associated in our minds with poisoning from infection with some micro-organism, that it may be considered an important point of resemblance between cancer and the infective diseases.

We shall now proceed to consider what positive evidence we have, both clinical and experimental, of the infectivity of growths, leaving the question of the infective agent itself for later consideration.

Accidental Inoculation in the Same Individual.

So many cases have been recorded of accidental inoculation in man, either from contact of a healthy surface with a cancerous one or as the result of operative interference, that we may regard the possibility of this occurrence as beyond doubt. Numerous examples have probably fallen under the observation of most surgeons. Such, for example, as the infection of the cheek from an epithelioma of the tongue, of one lip from the opposite one, of the vagina from the cervix uteri, and of a lower part of the alimentary tract from the upper.

Quite recently we have seen a small nodule of growth in the vagina near its lower end, evidently secondary to a carcinoma of the cervix, and but a short time ago an epitheliomatous mass on the alveolar process of the upper jaw opposite to and secondary to an epithelioma of the tongue. Cripps has recorded a case in which an epitheliomatous ulcer developed on the arm of a patient

suffering from ulcerated carcinoma mammæ, the ulcer occurring where the arm lay in contact with the breast growth. We need not give any further instances, as we are convinced this mode of infection is known to all.

Of *accidental inoculation as the result of operative interference*, many cases have been put on record. In cases given by Reincke and Quincke and others, carcinoma has developed in the punctures made to relieve cancerous peritonitis, and Sippel (quoted by Roswell Park) has published a case in which cancer inoculation took place along each one of the suture tracks after abdominal section for removal of a cancerous ovary. The same thing has been recorded several times as occurring in an abdominal scar after removal of ovarian tumours. In one case we have seen, in which the chin and lower jaw were split for the free removal of an epithelioma of the tongue, recurrence of growth took place in the centre of the scar in the chin.

The possibility of reinfection during the course of operation for malignant growth is now so well recognised by many surgeons, that if by any chance they cut into the growth they most carefully cleanse the wound and use fresh instruments, dealing exactly with the case as though it were infective in character.

A particularly instructive instance is quoted by Roswell Park, of a case under the care of a New York surgeon, in which carcinoma developed along the lines of incision made in the skin to relieve tension and allow the edges of a large wound resulting from an operation for the removal of a cancer to come together. These incisions were made with the same knife that had been used for the removal of the cancer, and the incisions had been in this way infected with carcinoma from the original growth.

Experimental Inoculation in the Human Subject.

Infection from one part to another of the same individual has not only occurred accidentally, but has been shown to be possible experimentally. Hahn, in 1886, cut out three fragments from an inoperable cancer of the breast, and transplanted them to the opposite breast. The woman died three months later, and a microscopical examination of the nodules which had developed at the site of inoculation showed that they had the same structure as the original tumour.

Cornil, in 1891, recorded a similar case, in which a successful transplantation from one breast to the other of a spindle-celled sarcoma was performed by some surgeon.

These cases of accidental and experimental inoculation can leave no doubt in our minds as to the possibility of reinfection of an individual from an existing cancer, and also provide us with a most useful object lesson of the results that may follow operations for removal, if the most scrupulous care in providing against reinfection is not attended to.

Bosc, quoted by Roswell Park, mentions that in three instances carcinoma has been experimentally communicated to healthy individuals, but he gives no details.

Infection from One Individual to Another and the Local Distribution of Cancer.

We now come to the question of infection from one individual to another, and with this is bound up the occurrence of cancer affecting many people occupying the same locality or house. Too many cases have been recorded now for us to look upon this in the light of coincidence. Even if we regard these as comparatively few in number, we must remember that it is only within the last few years that anything like prominent attention has been drawn to the subject. Further, the results that have attended the careful investigations of a few observers, such as Arnaudet, Wickham, Legg, and others, are most encouraging. One main source of difficulty in establishing the source of infection lies in our want of knowledge with regard to the incubation period. That the infection may remain latent for years in an individual, is shown by the long period that sometimes elapses between removal of cancerous growth and its recurrence either locally or in the glands. Moreover, it is not at present possible to say whether infection always occurs direct from individual to individual, or whether the causative agent is capable of retaining its vitality or even of undergoing some portion of its life history outside the body.

In spite, then, of the difficulties that beset us in our attempts to trace the source of infection, cases present themselves from time to time that are difficult of explanation on any other view than that of direct infection. A striking example is given by Dr. Hall

A Miss S. was operated upon on May 27th, 1885, for a tumour of the upper jaw, that was found microscopically to be a giant-celled sarcoma. She died as the result of the operation. Mr. J. A. was engaged to Miss S. for some months before her death. On September 9th of the same year (four months later) he had an operation performed for a tumour of the right superior maxilla, that, on microscopic examination, turned out to be a giant-celled sarcoma.

M. Guelliot has collected 77 cases in which there was strong reason to suppose that infection from someone previously affected had taken place, and it is worth noticing that in 71 of these the two affected were husband and wife.

One of us saw, some years back, a man with epithelioma of the tongue, for which the organ was excised. This man's wife had had her tongue removed some year or so previously for what was said to be cancer.

M. Guelliot gives numerous interesting examples. One case, communicated by M. Dève, was that of a man, aged 38, who was attacked with cancer of the tongue. He had been in the habit of smoking the pipes of his father-in-law, who had died some years back of an epithelioma of the face. A similar case is quoted by M. Fabre from the practice of Dr. Mollière.

Budd* has reported that five surgeons of the North Devon Infirmary, who died in succession of the disease, contracted it from their patients.

But still more remarkable examples have attended investigations upon the frequency with which cancer occurs in certain districts and even houses. D'Arcy Power has collected together many striking examples of localised cancerous outbreaks, and many others have been published both here and abroad. Space prevents us from giving more than one or two examples, but they will serve to indicate the character of the remainder. Dr. Roy (quoted by M. Fabre) relates a series of deaths from cancer, of people inhabiting the same house, in a village in the Ardennes. The husband died of cancer of the rectum in 1870, the wife of cancer of the breast in 1873, a servant of cancer of the breast in 1873, and the father-in-law of cancer of the rectum in 1875. Mr. Law Webb has put on record some equally striking cases. In one

* 'Lancet,' London, December 31st, 1887.

series, extending over a number of years, from 1866 to 1891, six people, who inhabited two houses under one roof, died of cancer.

Of the influence upon cancer mortality of certain geographical formations, we must refer to the work of Haviland and others. There seems to be no doubt that it is more frequent, as Haviland states, "in the shallow and low-lying vales traversed by fully-formed rivers, and having sites liable to floods, and composed of more recent geographical formations, in which clays of different ages predominate."

Thus locality is seen to be a distinct factor, but whether it has a predisposing influence by exercising some depressant effect upon the individual, or whether the organism is better enabled to live, or undergoes some portion of its life history outside the body in such districts, we are unable at present to say.

Whatever way we look at the matter, however, we can have but little doubt but that we are dealing with some *materies morbi*, and that, as D'Arcy Power says, those cases raise a strong presumption in favour of cancer being an infectious disease.

Experimental Work.

A large number of experiments have now been performed, to show the possibility of transference of growth from one animal to another, with a considerable amount of success, and some interesting results have thereby been obtained.

In most cases published the successful inoculations have been from animal to animal of the same species. Of the possibility of this, our own experience in the matter confirms the results of other observers. When, however, we come to reported cases of transference of growth, either from man to the lower animals or between animals of different species, the results of experiments are open to more doubt. The descriptions of the tumours obtained are often so meagre, or else could be equally well explained on the hypothesis that the deposits obtained were inflammatory in nature, that for the present we must regard the question of inoculation of animals of different species as not yet proved. One case quoted by Dr. Hall is, however, worth drawing attention to. A woman was in the habit of cleansing every day an ulcerated medullary sarcoma in an ox. She acquired a tumour on the fourth finger of the left hand, which Kuhn examined and found to be a medullary sarcoma.

Cases in which successful infection of the lower animals from man occurred have been reported by Langenbeck, Follin and Lebert, Goujon, Mayet and Bosc. Speaking of these experiments, Shattock says: "No authentic instance (that is, none satisfying modern criticism) is at present forthcoming of the grafting of human carcinoma upon any of the lower animals." He, in conjunction with Ballance, carried out a large number of experiments on these lines without obtaining any positive results. Ruffer and Plimmer were likewise unable to transfer carcinoma from man to the lower animals.

Though the large number of negative results obtained cannot be held to dispose of the few positive ones, it would suggest caution in the acceptance of any such recorded cases without the fullest details.

It is not difficult to obtain successful results from inoculation in animals of the same species. In 1876 Novinski made 27 inoculations of a growth, which he says was a carcinoma in a dog's nose, into subcutaneous tissues, with positive results in two cases. The growth that resulted resembled in structure that of the primary growth. Hanau's experiments with rats deserve special attention, as they appear to us to be the only undoubted examples of the transference of carcinoma. We have very little doubt that some of the other recorded cases of carcinoma are really sarcomata of an alveolar type. Hanau successfully transferred squamous-celled carcinoma growing in the vagina of a rat to a series of other rats. An illustration from a photograph of one of the rats, showing the infected peritoneum, is given in a paper by Shattock and Ballance, and these authors state that they have examined the sections, which were typical squamous-celled carcinoma.

A considerable number of experiments have been performed with the growths that occur not very infrequently on the genital organs of dogs, which we have elsewhere described as having the structure of round-celled sarcoma. These often present a well-marked alveolar arrangement, and this latter feature accounts for the facts that growths, apparently similar to the ones we have dealt with, have been described by other authors as carcinoma. Inoculation experiments have been carried out with these growths by Wehr, Geissler, and by Duplay and Cazin, portions of the growths being removed from the genital organs and transplanted

to the subcutaneous tissues of other dogs. In many cases tumours appeared at the site of inoculation, resembling in structure the original growths. In two of the recorded cases secondary deposits occurred in the viscera; a microscopical examination of the secondary tumours in one case showing that they possessed the same structure as the original growth.

We have carried out a considerable number of inoculations with the growths derived from the genitals of dogs. We have already published the details of the experiments, and shall consequently refer but shortly to what appears to us to be the most important points. The original growths on the genitals presented the structure of round-celled sarcomata, and their malignant character was shown by their tendency, if left alone, to infiltrate the vaginal walls, extending right through the muscular tissue and invading the perineum. And we may say at once that all the secondary tumours we obtained as the result of inoculation were identical in structure, nor was there any difficulty in distinguishing them at any time from inflammatory deposits.

In one batch of experiments we had 13 successful experiments out of 17 inoculations, tumours appearing at the site of inoculation. They ran a fairly rapid course, reaching a maximum of $1\frac{1}{2}$ to 3 inches in about three months' time. From this time onwards retrograde changes took place, and the growth, becoming soft and flabby, gradually disappeared in the course of another three months. In about half the cases this subsidence was accompanied by softening in the interior of the growth, with subsequent giving way of the skin and the discharge of a grumous material. It would appear that after the growth has existed for about three months the dog acquires some power of resistance to its further extension. That some immunity does actually exist for a time, though for how long we are unable to say, we have shown by experiments. Four dogs that had been successfully inoculated were again inoculated after the subsidence of the original tumours, two of them on two occasions each, with results that were uniformly unsuccessful, though other dogs inoculated at the same time, as a control, with material from the same source, gave positive results. This immunity is interesting as giving us another point of resemblance between new growths and diseases that we know to be due to some specific organism.

We found that the growths could be transmitted serially

through a number of animals. In one series the growth was transmitted through six dogs. There was no evidence, however, that the growth thereby altered in virulence or in any other particular.

For a considerable while we failed to obtain secondary deposits. The growths disappeared spontaneously, and we could not regard any of them as showing a malignant tendency. In one case, however, the growth ran a different course. Instead of subsiding at the end of three months, it continued growing, and the dog which had been thin and weakly throughout, died at the end of six months, the growths at that time being about 4 inches long on each side of the abdomen, in the subcutaneous tissue. At the *post-mortem* examination both the liver and spleen contained a number of small round nodules, varying in size from a millet-seed to $\frac{1}{3}$ inch in diameter. In structure these secondary deposits in the viscera were identical in all respects with the primary growth in the abdominal wall, namely, that of a round-celled sarcoma. No other cause for death was found, nor was there any evidence of inflammation or abscess formation about the viscera. That the tumours in this case continued growing and gave rise to secondary deposits, we attribute to the weakly condition of the puppy throughout, and to a lack of resisting power.

This case was important as showing us that the tumours resulting from inoculation were capable of presenting the same malignant tendency as the primary growths of the genital organs.

The benign course run by many of these growths, and their total disappearance, has its counterpart in man. Probably many of us present have seen growths which, from their clinical course, we have shrewdly suspected to be sarcomata, spontaneously disappear, and have then, in spite of our previous convictions, and perhaps in face of a histological examination, concluded that we must have been mistaken in our diagnosis. There seems to be little doubt, however, that malignant growths do occasionally disappear. We have seen a spindle-celled sarcoma of the abdominal wall subside, only to recur later in an unmistakable form. Mr. Bennett has placed on record a most instructive series of cases of this kind. The majority of them were sarcomata, one only being a carcinoma, the structure of the tumours being verified microscopically.

That the tumours in dogs resulting from subcutaneous inoculation should run the course they do is not to be wondered at. It is reasonable to regard healthy dogs as possessing some natural powers of resistance, and, in fact, as we have shown, some few could not be inoculated at all. It would appear that it is in highly-bred dogs and in weakly ones with but little resisting power that the growths show the characteristics of invasion, both locally and of remote parts.

Parasites in Cancer.

We will now consider what direct evidence we possess of the parasitic origin of malignant growths.

In the cells of malignant growths appearances are to be observed which have been attributed to the presence of parasites. Some of these appearances occur in other conditions, and are caused by degeneration of the protoplasm; but, after eliminating such errors, the fact remains that in the cells of malignant growths, especially in carcinoma, bodies are present which are not found in other lesions, and which have the same morphology as micro-organisms belonging to the lower animal and vegetable kingdoms.

As far as their microscopical characters are concerned, they *may* belong to the class of protozoa or to the class of blastomycetes. It has for a long time been known that certain protozoa are pathogenic to animals—for instance, the coccidia, which give rise to papillomatous growths in the liver of rabbits; and, by analogy, the bodies found in cancer cells were considered to be protozoa. But these bodies, from their microscopical appearances, may equally well be blastomycetes, and the tendency nowadays is to consider that they are of this nature.

It is obvious that histological methods are insufficient to determine what these bodies are. We will see how far experimental methods assist us in solving the problem.

The blastomycetes are micro-organisms belonging to the vegetable kingdom, of which a well-known example is the ordinary yeast plant. There are a large number of different kinds, which have been isolated from various sources, and the pathogenic properties of which have been studied.

Several have been shown to be the cause of disease in the human subject, such, for instance, as the one described by Gilchrist and Royal Stokes, which produced a chronic inflammatory affection

of the skin resembling lupus; and the one described by Busse, which produced a chronic form of pyæmia.

Sanfelice isolated a blastomyces from fermenting fruit juice, to which he gave the name *Saccharomyces neoformans*, and which was pathogenic to animals.

In guinea-pigs inoculation led to a general infection, and the micro-organisms could easily be cultivated from the tissues. Nodules were found, *post mortem*, at the site of inoculation and over the peritoneum, consisting of accumulations of parasites, with a few cells and a little connective tissue.

Dogs were more resistant, and in a large proportion of cases the result of inoculation was negative. The most important experiments he has recorded were the inoculation of two dogs, one into the breast and the other into the testis.

The dog inoculated in the breast developed a tumour which microscopically had the structure of a duct or tubular carcinoma, the tubules being lined with columnar epithelium. In the adjacent lymphatic gland there were deposits of similar structure.

The dog inoculated in the testis developed a large mass, in which it was impossible to discover any testicular structure. Around this mass there were a number of metastatic nodules, and there was a large one in the neighbourhood of the glans penis. The tumours, on microscopical examination, were found to have the structure of an adeno-carcinoma.

In another series of experiments Sanfelice injected cultivations of the *Saccharomyces neoformans* into the veins of dogs. Nodules, consisting of cells, which apparently were mesoblastic, were found in the kidneys. In the lesions in the dogs a few parasites were found on microscopical examination, but they could not be cultivated. Sanfelice explains this by supposing that the *Saccharomyces* exists in two phases, in one of which it cannot grow outside the tissues of an animal.

It would appear from these experiments that the same organism is capable of giving rise to a tumour having the structure and behaviour of carcinoma, when inoculated into epithelial structures, and of giving rise to nodules derived from proliferation of connective tissue cells, when the mesoblastic structures alone receive its stimulus.

Plimmer cultivated from a human carcinoma a blastomyces, which, when inoculated into the scarified surface of the cornea

of rabbits, caused epithelial proliferation, and when inoculated into the peritoneal cavity of guinea-pigs gave rise to transparent nodules in the peritoneum, lungs, liver, spleen, and intestines. He describes these nodules as consisting of masses of endothelial cells with a round-celled infiltration around.

These experiments, combined with those of Foulerton and other observers, prove that several of the blastomycetes are pathogenic for animals. In most instances the lesions produced are of an inflammatory nature, in some cases with a preponderance of the fixed connective tissue cells. But from Sanfelice's description of the inoculation of dogs with the *Saccharomyces neoformans* an epithelial growth with malignant characters has been produced. If these experiments are confirmed by other observers, they will have the most important bearing upon the causation of cancer.

A number of observers have cultivated blastomyces from cases of cancer in the human subject. Most of these micro-organisms were non-pathogenic to animals, and it is probable that they were accidental contaminations. After a careful perusal of the literature, we do not feel satisfied that parasites, existing during life, have yet been isolated from cancers in the human subject.

À priori, there is nothing remarkable in a parasite causing a primary epithelial or mesoblastic growth. We know that bacteria may give rise to proliferation of epithelium and fixed tissue cells.

However, in malignant growths we have to explain why secondary deposits are of the same structure as the primary growth, independent of the tissue in which they are deposited. This can only be explained on the supposition that the cells and the parasites are together conveyed to distant parts, and that the cell is able to multiply in virtue of the presence of the parasite.

The fact that cells when transplanted are capable of limited multiplication, renders this view more easy to understand.

We may summarise the facts that we have now brought before you as follows :—

1. Malignant growths may be regarded as local in origin, and as possessing the power of infection of adjacent and distant parts of the individual.

2. Inoculation may take place from one part to another of the same individual, apart from transference by the natural channels.

3. There is good evidence to show that one individual may be infected with growth from another

4. There is experimental evidence to show that growths may be transferred from animal to animal of the same species by inoculation.

5. There are found in many malignant growths bodies which have a resemblance to micro-organisms, and which have been regarded as belonging either to the protozoa or the blastomycetes.

6. A new growth, having the structure and also the behaviour of carcinoma, has been described as arising at any rate in two instances, from inoculation, with a form of blastomyces.

7. These experiments are highly suggestive that the bodies found in cancer are the cause of the disease, though the evidence is wanting that definitely associates the two.

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Mr. Jackson Clarke agreed with the authors in describing the preparations as typical sarcomata. Dr. Washbourn had supplied him with two kinds of sections, one being exactly like human growths of typical alveolar sarcoma, and the other a round-celled sarcoma. The evidence of the spreading of these tumours in the more weakly of the subjects experimented upon was a point of great interest. The authors had referred to Dr. Lack's recent observation of one case of cancer-like growth in a rabbit, and in that case the nature of the experiment—an emulsion of ovarian cells injected into the peritoneum—made it very akin to an implantation experiment. Little groups of cells from the follicles of the ovary would be readily taken up by the ostea of the peritoneum, and thus find their way to the lymphatic glands, so that it was impossible to say, in this case, whether there had been any spreading. That experiment, therefore, did not elucidate the nature of the cancer as being simply of the nature of epithelioma. They had to imagine something which scattered the epithelium as the author did with his scalpel. In going over the subject Mr. Jackson Clarke found that some of his earlier conclusions were erroneous ; others he still could not explain on any hypothesis except that he had originally put forward. His basis was purely histological, on the analogy of structure between the sporozoa and the forms met with in neoplasms occurring in man. Some of his proofs he ultimately thought were unsound, so that he was still in a state of indecision. Since then the work of certain observers in this country and in Italy had been of special interest to him. The forms to which he had alluded as not being explainable, unless as parasitical in human growths, he could not ascribe to blastomycetes. He had often found various bacteria in nodules of new growth far removed from the surface, and he imagined them to have been accidental contaminations which had been conveyed along the lymphatics with the embolism of the original growth. He used to have an idea that these malignant tumours might be comparable to the tertiary growths of syphilis, and might ultimately turn out to be connected with some of the common fevers. He concluded with the remark that the communication to which they had listened was the result of solid and, to some extent, novel observation.

Dr. Symes Thompson related two instances which he thought might have some bearing on the subject, though he admitted they might be simple coincidences. The first was that of two men who had married two sisters, but who were not otherwise related, who both developed epithelioma of the tongue. One man never smoked, and the other only smoked cigars, so that factor could not be operative. The other case was one that he had seen that day of a gentleman who six or seven years ago had a disease of the tongue which was thought to be syphilitic, or possibly an epithelial cancer. The growth was removed but its nature had not been clearly made out. Two years later the disease returned in the tongue, half of which was excised. Then for several years the patient was free, but ultimately a tumour formed in the neck on the same side as the affection of the tongue, and this was found to be of a sarcomatous type. After an interval of 18 months a second sarcomatous tumour formed lower down the neck, and this had just been successfully removed. He commented on the occurrence of the two kinds of growth in the same person, and suggested that at least the localisation might have had something to do with the first growth, although the nature of the two was so different.

Dr. Heywood Smith said it seemed to be inferred in the paper that

the convection of cancer was of the nature of auto-infection, *i.e.*, it was easier to transfer from side to side of the same body than to convey it to another body. They had, however, scarcely ever heard of cancer being conveyed from one person to another. Clinically, he referred to a case under his care 25 years ago. He had operated, in hospital, on a girl, aged 14, for double malignant disease of the ovaries, the temperature at the time of the operation being 104° F. In those days antiseptic precautions were not in vogue, and, for some reason, the parts removed, or the fluid from the peritoneum, were left in the theatre for two or three days. About four days afterwards he operated upon a woman for ordinary ovariotomy. She made a good recovery after the operation, but two or three weeks later she became ill and died within two months. *Post mortem* they found a large malignant mass occupying the abdomen, which, at the time of the operation, was perfectly healthy. There were also deposits in the pleura. In thinking over that case, he had been unable to resist the conclusion that the infection must have been conveyed by the instruments or through the air.

Dr. Lambert Lack said that cases similar to the one related by the last speaker were by no means unknown or even uncommon. He believed that in these cases the healthy peritoneal cavity was inoculated with epithelial cells existing in the fluid of the cyst. If his (the speaker's) experiments were correct, such a result was to be anticipated when, during the operation, fluid from a cyst was permitted to escape into the abdominal cavity. Dr. Lack considered the infectivity of cancer hard to reconcile with the parasitic theory. The authors had offered no explanation of such cases as those in which a squamous epithelioma of the buccal region had been followed by a similar epithelioma in the stomach or intestines. Surely, if a parasite had conveyed the infection the secondary epithelioma would have been of the columnar-celled variety. Certainly these cases could only be due to transplantation of the cancer epithelium, in fact all secondary growths in cancer are best explained in this way, and form a strong objection to the parasite theory of cancer.

Dr. Heywood Smith (referring to a remark by Dr. Lack) said that they had all seen instances of infection of the peritoneum from rupture of an ovarian sac. What he had said was, that there was but little evidence of the infection spreading from one individual to another.

Dr. Auld said he had seen columnar epithelioma of mucous membrane, under conditions of irritation, recur as squamous epithelioma. It all depended upon the conditions present at the site of transplantation. He believed they might have a columnar-celled infective tumour infecting a mucous membrane and producing a squamous-celled growth, if the conditions of the mucous membrane at the site of the growth were such as to bring about that particular kind of growth, those conditions being probably very much governed by the cells of the mucous membrane in question. He mentioned that in a case of columnar-celled growth of the trachea, the cells, after being attacked by fresh irritation, reproduced themselves as squamous cells, so that it was impossible to say that the infection necessarily reproduced itself as columnar-cells, or to draw a hard and fast line as to the particular shape of the epithelium in every case.

Professor McFadyean said it might appear ungracious on his part to criticise so admirable a paper, or to suggest that there was any defect in the way in which the subject was handled, but it appeared to him that

in discussing the question of the etiology of malignant neoplasms it was highly desirable to separate the connective tissue from the epithelial groups. Probably, every pathologist was prepared to admit that the tumours they at present included under the head of sarcoma contained things that represented the reaction of the tissues to living organisms. The cells of sarcomata, of whatever type, were mesoblastic in origin, and there was nothing so dissimilar in the structure of sarcomata and many bacterial growths as to justify them in saying it was *à priori* improbable that at least some of the former were due to bacteria. He would only recall the fact that in veterinary pathology the tumours now known to be due to the actinomyces were formerly described as sarcomata. He thought the authors had discussed the etiology of the carcinomata with a recognisable bias towards the theory of infection; at any rate, he hoped they would forgive him if he said that they had sailed rather lightly over some of the principal objections to the view that malignant tumours in general were caused by parasites. One of the great difficulties was this constant association between the epithelium and the hypothetical parasite. It had been said that infection only took place when the parasite was transported along with the epithelium which it had invaded. He asked whether any analogous instance could be adduced in regard to lesions which were known to be parasitic in their nature. In tubercle of the liver, for instance, if they took some liver cells that were involved in the process and injected them into the body along with the tubercle bacilli, they did not find that the liver cells underwent multiplication. He pointed out also that there were other malignant epithelial tumours besides the epitheliomata—to wit, the adenomata. Even comparatively highly-organised adenomata were occasionally malignant, and he did not see how a parasitic irritant could give rise to a new growth which simulated the structure of a gland. Besides, there was a gradation between the adenomata and the carcinomata, and one might merge into the other. He foresaw another great difficulty which would be raised by assuming that the parasites were infective only when transported with the infected epithelium. That would compel them to admit that the hypothetical parasite of cancer was an obligatory parasite. He ventured to say that it was simply inconceivable that cancer was transmitted solely by contagion or infection from individuals, and, that being so, they were compelled to suppose that the hypothetical germ must be able to live outside the animal body, and, therefore, independent of epithelium. A similar difficulty presented itself in the case of some of the sarcomata. For example, assuming that the melanotic sarcomata were parasitic in their origin, why were the metastic tumours always composed of pigmented cells? He added that if one took into consideration the occurrence of carcinoma in the lower animals one found very little support to the parasitic theory. Carcinoma of the mammary gland of the cow was absolutely unknown, so far as he was aware. That was not what one would have expected if it were due to parasitic infection. Many cases of carcinoma in the human subject were ascribed to traumatism, but it was a singular thing that this had never been observed in respect of carcinoma in the horse. If cutaneous irritation or friction had anything to do with the production of carcinoma one would expect to see it constantly cropping up at the seat of saddle-galls, &c., but he had never seen a carcinoma develop at any of the seats of friction by harness in the horse.

Dr. Washbourn, in reply, said, in reference to Dr. Lack's observation, the chief difficulty he found was that only one experiment had been made, so that many errors might have arisen. The chief error was that the ovary which he used for this experiment was simply disseminated by breaking it up and scattering it in the abdominal cavity. Then also, in this single experience, it might have been a pure coincidence that the animal died of cancer. The second case referred to by Dr. Symes Thompson was, he thought, capable of explanation—the case in which a man had an epithelioma of the tongue, and subsequently a sarcoma in the neck. Sanfelice stated that by inoculation with blastomycetes he produced growths having the nature of carcinoma according to the site of inoculation. When inoculated into the breast or testis, a growth was produced with the characters of a carcinoma ; while if the same organism were inoculated into the veins, nodules were produced in the kidneys, which on microscopical examination appeared to be of mesoblastic origin ; so that the same organism could produce epithelial and connective-tissue growths. Possibly the same micro-organism might produce an epithelial growth of the tongue and a sarcoma in the neck. The transference of cancer of the ovary to another individual during operation, mentioned by Dr. Heywood Smith, was of great interest. They had brought forward several instances of probable transference of cancer from one individual to another. The question had been raised as to the difficulty of explaining why a secondary deposit is dependent upon the tissue from which it arises, and not upon the tissue in which it occurs. They had pointed out in their paper that this could only be explained by supposing that the epithelial or connective-tissue cells were conveyed together with the parasite. If the parasite alone passed from the primary growth into the organs, it would not necessarily produce a growth having the same structure as the one from which it was derived ; but if parasite and cell were transferred together, the latter might continue to multiply, and would produce tissue of the nature of that from which the cell was derived ; and they knew, from transplantation experiments, that the cells had a certain power of multiplication. Professor McFadyean had criticised their views, but had not expressed his adherence to any particular theory. It was possible that some micro-organisms might produce a different reaction to that produced by others ; the coccidia, for instance, gave rise in the rabbit's liver to papillomatous growths. The last speaker had called attention to the rarity of carcinomatous growth in the udder of the cow. Might he point out that veterinary pathology had not yet reached its acme, and that possibly further observation on these matters might be forthcoming in years to come ? He said this because the sarcomata which they had described as occurring in dogs were barely alluded to in the various veterinary works which they had consulted—at any rate, as far as the structure of the tumours was concerned. On going into the question, however, they found that it was fairly prevalent, and was well known to dog-fanciers. He hoped, therefore, that further observations would soon be forthcoming of the occurrence of cancer in animals.

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PROGNOSIS IN APPENDICITIS, WITH AN ILLUSTRATIVE SERIES OF 200 CASES.

By H. A. Caley, M.D. Lond., F.R.C.P.

During the last few years so much has been written and spoken about disease of the appendix, that one hesitates to contribute another paper on such a well-worn theme. The subject of prognosis, more particularly the basis of prognosis in individual cases, is, however, by no means threshed out, and my object will be attained if the points now raised induce discussion as to the relative value of the available data. The discussion of questions of treatment is intentionally avoided, but a consideration of prognostic indications will serve indirectly to emphasise the essential importance of regarding "appendicitis" not as a defined entity, but as a group of conditions ranging from acute perforative appendicitis at one extreme, to a mild perityphlitis at the other, and of recognising that careful differentiation is the only sure foundation of rational treatment—treatment based on the actual conditions present, and not on routine lines, surgical or medical.

Any acute attack of appendicitis involves more or less immediate risk to life during the attack, and there is the well-known liability to recurrence at a subsequent period, so that the subject of prognosis may be considered in two sections: (1) prognosis as to recovery from the attack, and (2) prognosis as to subsequent recurrence.

1. Prognosis as to Recovery.

General statistics are, in the case of affections of the appendix, more than usually untrustworthy, and are entirely valueless unless the survey is sufficiently wide to include all the forms of appendicitis, and unless care is taken to distinguish clearly between case mortality and attack mortality. The importance of including every type of case, mild or severe, is brought out in the series of 200 consecutive cases treated in the medical and surgical wards of St. Mary's Hospital, which are here adduced to illustrate certain points. I desire to express my indebtedness to my colleagues, Dr. W. B. Cheadle, Dr. D. B. Lees, Dr. S. Phillips,

Mr. E. Owen, Mr. H. W. Page, Mr. A. J. Pepper, Mr. A. Q. Silcock, Mr. J. E. Lane, and Mr. H. S. Collier, for permission to include in the series cases under their care, many of which I have had opportunities of examining from this point of view. It must be clearly understood that any statistics here given are intended solely to illustrate certain points, and are not to be regarded as in themselves a basis for deduction.

Ninety-eight cases were treated throughout in the medical wards, with three deaths; and 102 were treated in the surgical wards, with 22 deaths, a certain proportion of the surgical cases having been transferred from the medical side. The low mortality of the medical series is attributable to the large proportion of cases of simple perityphlitis (appendicitis with local non-suppurative peritonitis), just as the higher mortality of the surgical series is due to the large proportion of cases with general peritonitis, which was present in 20 out of 25 fatal cases. When the two sets are grouped together we have a more representative series, showing an aggregate mortality of 25 in 200 cases, or $12\frac{1}{2}$ per cent., a figure which bears a general correspondence with the 14 per cent. given by Mr. F. Treves as an approximate average for hospital cases. It is certain that hospital statistics give an exaggerated idea of the tendency to a fatal termination, for the reason that many of the milder cases are not sent to hospitals, while the more severe forms, with general peritonitis, gravitate there, as the hospital surgeon, who is called upon to operate on practically hopeless "too late" cases, knows so well. Mr. Treves considers that if a large series, including both hospital and private cases, were taken together, the mortality would be nearer 5 per cent. than 10 per cent.

Of much greater importance, however, than percentage mortality in the aggregate is the rate of mortality in the several types—a point emphasised by Dr. H. P. Hawkins in his instructive series of 264 cases from the records of St. Thomas's Hospital. The necessity for careful differentiation lies at the very root of accurate diagnosis, and therefore of prognosis; and any classification, if it is to illustrate these points, must be based upon the relationship between the lesion of the appendix and any resulting peritoneal or other infection, and not upon the pathological features of the lesion of the appendix by itself. In the following table the cases are grouped into five classes:—

Class.	——	Recovered.	Died.
1	Simple appendicitis and perityphlitis ..	99	—
2	Appendicitis with localised suppuration (operation in 27 cases)	29	2
3	Appendicitis with diffuse peritonitis (operation in 17 cases)	3	20
4	Chronic and relapsing appendicitis (operation in 37 cases)	42	—
5	Appendicitis with special complications (operation in 3 cases)	2	3
		175	25
		200	

Class 1, simple appendicitis and perityphlitis, comprises a minority of cases in which the inflammation is almost or entirely limited to the walls of the appendix, and a larger number of cases in which, as the result of the appendicitis, there is simple perityphlitis, *i.e.*, localised non-suppurative peritonitis of an adhesive type. (Cases of perityphlitis, due to lesions of the cæcum, are not specially considered, the proportion due to cæcal and not to appendix lesions being so small as not to be of material consequence in relation to prognosis.)

Class 2 consists of cases in which the appendicitis has resulted in localised suppuration, usually intra-peritoneal, sometimes retro-peritoneal.

Class 3 comprises all cases of appendicitis resulting in diffuse as distinct from localised peritoneal infection. The term "diffuse peritonitis" is employed rather than "general peritonitis," because not rarely the peritonitis is limited, or nearly limited, to the lower half of the abdominal cavity.

Class 4 is made up of chronic and relapsing forms. Any case in which three or more attacks occurred is included in this class. The individual attacks of relapsing cases resemble those of Class 1, but from the pathological and prognostic standpoints they have definite differential features.

Class 5 is a small and diminishing group consisting of cases in which the chief danger arises from complications other than those directly connected with peritonitis or local abscess, *e.g.*, intestinal obstruction, fæcal fistula, suppurative pylephlebitis with portal or general pyæmia, hepatic or subphrenic abscess, thrombosis of the iliac vein, pleurisy and pneumonia, parotitis, or secondary hæmorrhage.

In the 200 cases of my series there were two of acute intestinal obstruction, three of fæcal fistula (all of which did well), two of hepatic abscess, one of subphrenic abscess, one of parotitis, one of acute pulmonary œdema after operation, and one of pneumonia; but only those in which the special complications influenced their course are included in Class 5.

With these general observations we may now pass on to the consideration of the principles of prognosis in an individual case. Prognosis must ultimately depend upon the precise nature of the lesion, which, in fact, constitutes the pathological basis of prognosis. Corresponding with the morbid processes we have various clinical manifestations, and the latter, in so far as they afford indications of what is actually taking place, constitute the clinical basis of prognosis.

The Pathological Basis of Prognosis.

There are two essential factors in the pathological basis of prognosis: 1. The nature and course of the lesion of the appendix regarded from the point of view of relative infectivity. 2. The extent and nature of any resulting peritoneal infection, which depends for the most part on the actual condition of the appendix and in part also upon its position and relations, and upon any special modifying conditions, such as the presence of old adhesions. The most important direct prognostic factor is the peritoneal, and it may therefore be considered first.

1. *The Extent and Nature of the Peritoneal Infection.*—It is well known that, given a case of appendicitis, the danger to life in a very large measure hinges upon the extent and nature of the peritoneal involvement or liability to involvement. In the large majority of instances when the disease is fatal the cause of death is diffuse infection of the peritoneum resulting in septic peritonitis. Thus, in 20 out of 25 fatal cases in my series there was diffuse

peritonitis, frequently associated with local collections of pus in the vicinity of the appendix. It will be noticed that in the present series of cases the mortality in those with diffuse peritonitis was approximately 87 per cent. Many of these patients were in a practically hopeless condition when they were admitted. Some no doubt, perhaps many, might have been saved by early operation. It will not be appreciably beside the mark if we conclude that where there is diffuse peritonitis the mortality will range between 80 per cent. and 90 per cent.—a conclusion which forces into prominence the necessity for attempting to anticipate by early operation such widespread infection.

Next to diffuse septic peritonitis, and putting aside such rare and fatal complications as suppurative pylephlebitis or hepatic abscess, the most formidable condition is local suppuration. It is, however, in connection with this group of cases that the modern treatment of appendicitis has produced the most conspicuous benefit. For example, in my series, out of 31 cases with strictly localised suppuration 29 recovered, and it so happened both of the fatal cases were obscure cases with secondary abscess of the liver, and in one of them general pyæmia also. In two out of the 31 cases the abscess ruptured into the bowel and was thus evacuated.

Turning to Class 1 we see that when the lesion is of a strictly localised and relatively non-infective type the prognosis, so far as immediate risk to life is concerned, is highly favourable—in this series and in Dr. Hawkins's series uniformly so. Whilst, however, the immediate risk to life from a localised adhesive peritonitis is very slight there are dangers of a more remote kind. Adhesions may persist, leading to intestinal obstruction, which was the cause of death in two of the cases in Class 5, or permanent structural changes may be started in the appendix which sooner or later give rise to recurrent attacks—usually, it is true, only slight attacks, but occasionally, owing to the supervention of intense infective inflammation, of a grave type. Of Class 4, composed of chronic and relapsing cases, it is only necessary to point out that the individual attacks were for the most part of a mild type, and that of 38 cases in which the appendix was removed all the patients recovered except one, who was an elderly man suffering from chronic appendicitis. He died three days after the operation from acute pulmonary œdema.

This case is included in Class 5. Such facts as these are sufficient to illustrate that so far as immediate risk to life is concerned the chief factor is the extent and nature of the peritoneal infection, whether localised or diffuse, suppurative or non-suppurative, of mild or severe degree of virulence.

2. *The Nature and Course of the Appendix Lesion.*—From the point of view of prognosis we may divide appendix lesions into severe forms and mild forms on the basis of relative infectivity. Some forms of appendicitis may be regarded as mildly infective, others as severely infective, the distinction depending in part upon gross anatomical changes, and in part upon the virulence of the infective organisms contained therein. As to the latter we have no certain data available for purposes of prognosis; on the other hand, it is comparatively easy to gauge the relative severity of the anatomical lesions of the appendix. Severe lesions of the appendix are: (1) Perforation, whether from concretion, foreign bodies, ulceration, or gangrene; (2) gangrene (whether from interference with blood-supply or from intense infective inflammation); and (3) ulceration, due to specific infection, *e.g.*, tuberculosis, typhoid fever, dysentery, or actinomycosis, the presence of concretions, or the result of intense inflammation. Perforation, gangrene, and ulceration are those which possess in the greatest degree the property of infectivity, but to these we must add two others; (4) concretion; and (5) suppuration within the appendix itself, which, though they may exist for a long time without causing trouble, are potentially severe lesions. These severe lesions produce a relatively intense infection so that the outcome is in the most grave cases diffuse septic peritonitis, and in the less grave, but still severe cases, local suppuration. For example, of 18 cases of diffuse peritonitis 12 cases were associated with perforation and six cases with gangrene, both perforation and gangrene being in many instances secondary to concretion, and 10 out of 31 cases of local abscess were proved to be due to perforation or gangrene; whilst in many of the remainder there was strong reason to suspect perforation. Mild lesions of the appendix are: (1) Simple inflammation ("catarrhal," or more correctly "parietal," appendicitis), acute or chronic; (2) stenosis or obliteration of the lumen, which is frequently associated with (3) cystic dilatation of distal portion; (4) concretion without ulceration: (5) superficial ulceration; and (6) kinking, adhesion,

volvulus, or hypertrophy of mucosa, any of which may occlude the lumen. This group, while it consists of lesions which are comparatively mild from the point of view of acute peritoneal infection, comprises most of those which are responsible for chronic and relapsing cases. Of 24 cases of my series in which the appendix was excised in the interval between the attacks, and its condition carefully noted, in nine cases the walls were greatly thickened, in seven cases there was stenosis or obliteration of the lumen, with cystic dilatation of the distal end in five out of the seven, in six cases extensive adhesion with or without kinking, and in two cases only was a concretion present. It is noteworthy that in most of these cases the lesion was such as directly or indirectly to encroach upon the normal lumen of the appendix.

The Clinical Basis of Prognosis.

The uncertain and often treacherous course of appendicitis, with the insidious development of grave complications, is a matter of common knowledge. Much stress has been laid on the impossibility of ascertaining early in an attack whether resolution or suppuration will be the outcome, but on pathological grounds it is clear that, from the prognostic standpoint, this is not the most important point. The important questions in the first instance are : Will the peritoneal infection be localised or diffuse ? Is it a mild attack, or is it one of the grave forms of appendicitis with which we have to do ? To determine prognostic indications we must proceed on the same lines as we do in order to ascertain diagnostic indications, and in their characters and relative significance, taken as a whole, we have the basis of prognosis. There is no single indication which may not mislead, but the sum of all the indications will not usually do so. These may be conveniently grouped under four headings : (1) local symptoms, (2) general symptoms, (3) local signs, and (4) the progress of the case.

1. *Local Symptoms.*—(*a*) Pain is one of the most constant of symptoms, and though we may have intense pain in what develops into a mild case, and only slight pain in a case of gangrenous appendicitis which proves rapidly fatal, it is none the less true that the distribution, severity, and character of the pain are of much significance. It is, of course, common for the initial pain to be referred to the umbilical region or elsewhere, rather than

to the region of the appendix, but in mild cases it tends to become localised in the right iliac fossa, whereas in perforative cases the pain is usually more sudden, more severe, and more generalised. It is when pain is associated with definite collapse that it assumes its maximum importance and suggests perforation, rupture of adhesions, or rapidly-spreading peritoneal infection, and we must not be misled by an interval of apparent quiescence following the onset of the acute pain. In some cases we have two distinct phases of pain—a preliminary dull aching pain or slight colicky paroxysms for a day or two, and then a sudden increase in severity, the more acute pain being usually accompanied by vomiting and general disturbance. Such preliminary pain may coincide with the development of local inflammation of the appendix itself (the pre-peritoneal stage), the more severe pain with the onset of acute inflammation around the appendix (the stage of appendicular peritonitis), or, again, an increase of pain may be the result of spreading peritonitis, or may be attributable to the rupture of an encysted abscess. In any case a sudden increase of pain should attract anxious consideration, and may be an urgent indication for immediate operation. The duration of severe pain is also of much significance, since in the majority of mild cases it soon tends to become more localised and less intense. Persistent severe pain—*e.g.*, pain so severe as to render necessary the continuous administration of morphia or opium—generally means a severe lesion; and if the pain, in addition to being persistent and severe, radiates to the left iliac region or upwards, it is suggestive of spreading peritonitis. Persistent severe pain which is strictly limited to the region of the appendix is, I think, especially apt to accompany concretion. Pain which is materially increased by deep respiration is suggestive of considerable local peritonitis, and the same may be said of marked pain or difficulty on micturition. No one who has carefully compared the duration, character, and course of pain in the mild attacks of relapsing appendicitis with what occurs in the acute perforative cases can question its significance, though it must be clearly understood that the converse of this is not necessarily true, since there may be a very severe lesion without much pain, particularly with gangrene of the appendix. (*b*) Vomiting, if present, usually accompanies or follows the pain, sometimes it precedes it. It is not uncommonly absent in the mildest forms,

and in the majority of average cases it is an initial symptom only, and ceases when the patient is kept absolutely at rest in bed and carefully dieted. If vomiting is frequent and severe, and especially if it is persistent, it is a valuable indication of a severe lesion, and the same holds good of vomiting which recurs after a period of comparative repose. Of 93 cases of my series in which this symptom was carefully inquired into, in 64 cases it was present at or soon after the onset and then ceased, and of these 59 recurred and five died; in seven cases vomiting was persistent and severe, and of these seven five died; and in 29 vomiting was not noted, and all of them recovered. Occasionally the character of the vomit may be significant—*e.g.*, stercoraceous or coffee-grounds vomit, both of serious import. Although as a positive indication the presence of vomiting is important, its absence proves nothing—*e.g.*, in gangrenous cases with general peritonitis it may not be present at all; but such exceptional occurrences do not detract from its general significance—a significance which, I think, is rather under-estimated. Constipation, not uncommonly preceded by diarrhœa, is so usual in all types that in itself it is not of any differential import; but, other things being equal, a natural action of the bowels and ability to expel flatus are favourable. Hiccough, especially if persistent, is unfavourable.

2. *General Symptoms.*—To help us in determining how far the local lesion is affecting the organism as a whole, we have two reliable guides: (*a*) the temperature, pulse, and respiration, considered conjointly, not separately—a point upon which I would lay very great stress; and (*b*) the aspect and general condition of the patient, with especial reference to collapse or anything approaching it, restlessness, or general distress. (*a*) Temperature. It is generally recognised that, taken by itself, the temperature chart in appendicitis is apt to be a fallacious guide; if, however, it be considered in conjunction with the frequency and characters of the pulse and the frequency and type of respiration, we are not likely to be seriously misled. Appendicitis is typically a febrile affection, but there are at least four conditions in which the temperature may be normal or subnormal: (1) the initial collapse of acute perforative or gangrenous appendicitis; (2) fully-developed septic peritonitis; (3) local suppuration, the abscess being strictly circumscribed; and (4) mild appendicitis without

appreciable local peritonitis. Subject to such exceptions as these, the range, type, and duration of the pyrexia have special significance. High fever of a continued type during the first two or three days generally means a severely infective lesion, but it does not tell us whether the infective process will be local or diffuse, and at times fever of this kind accompanies cases which undergo rapid resolution. A fall of temperature, if accompanied by a fall in the pulse frequency and by improvement in the general condition, is usually reassuring, but not otherwise. A secondary rise of temperature after the first few days has a very definite meaning —spreading peritonitis, suppuration, or some intercurrent complication—whilst fever persisting after eight or ten days generally means local abscess, in which case, if allowed to continue, it will usually assume a more intermittent or remittent type. The pulse is, on the whole, of more prognostic value than the temperature, provided that its characters as well as its frequency are carefully noted, that relative rather than absolute frequency is relied on, and that a careful distinction is drawn between a persistent and a merely temporary alteration. A high pulse-rate suggests a severe lesion, and in particular diffuse peritoneal affection, the more emphatically so if the pulse frequency is disproportionate to the temperature. I find that in the large majority of the less severe cases included in this series the pulse-rate was between 80 and 100; in every one in which it was persistently above 120 the attack was a severe one, and in most of them terminated in local suppuration or general peritonitis. It is true that in two or three cases, when *in extremis* from general peritonitis, the pulse-rate was not above 100, and in one it was below that point; but this is only what may occur in the later stages of any infective disease, and does not detract from the general significance of a high pulse-rate. The suggestion, which by some recent writers is treated as almost axiomatic, that a pulse-rate of 120 is an indication for operation, has much evidence to support it, but we must not forget that to wait for any such development may be to let slip the best time for operating in an acute case. Frequency and type of respiration are chiefly of importance in correcting an erroneous inference from a low temperature and pulse-rate in what may actually be a desperate condition, frequent shallow respirations of the costal type constituting, under such circumstances, a warning of grave peritoneal

implication. (*b*) Aspect and general condition: The importance of the pinched and anxious aspect, of restlessness, obvious distress, or collapse, are too well known to require detailed reference. Speaking generally, in appendicitis such symptoms mean acute peritoneal shock or general peritonitis. The initial collapse which at times marks the onset of perforative appendicitis may be so transient that shortly afterwards the patient's general state gives no idea of the gravity of the case, but the fact of its having occurred is none the less significant. Occasionally in the most rapidly fatal cases there may be more or less persistent collapse, and the development of any such symptoms in the course of a case is naturally of the worst possible omen. Of the third group of prognostic indications it is only possible to speak very briefly.

3. *Local Signs.*—The condition of the abdomen as ascertained by careful physical examination. (*a*) Impaired mobility during respiration, local or general, and distension, local or general, frequently go together, but in appendix cases the former is more reliable than the latter. It is quite exceptional for there to be no impairment of movement during respiration, whereas obvious distension is frequently absent throughout. Local immobility, with or without slight fulness, is the rule in cases of appendicitis with localised peritonitis. General immobility, with one important exception, points to the onset of diffuse peritonitis. The exception is diaphragmatic immobility, which appears to be due to simple reflex inhibition. This may be met with in any case in which the initial pain is very severe; it is usually transient and passes away when the pain is relieved by sedatives. With this exception we may regard the extent and degree of impairment of movement as one of the most reliable data in estimating the amount of peritoneal implication, and, therefore, as one of our most valuable prognostic indications. (*b*) Local tenderness: The area of local tenderness affords some guidance as to the extent of the local inflammation, provided that the patient's sensibility is not dulled by opium or by collapse. When there is little or no circumjacent peritonitis the tenderness may be strictly limited to the region of the appendix, *e.g.*, in chronic appendicitis and in the early stages of acute appendicitis. In most cases of perityphlitis with considerable local peritonitis there is a considerable area of tenderness in the right iliac region with its maximum intensity over the appendix, whilst in the more severe

forms with widespread peritonitis we find marked tenderness in the left iliac fossa and possibly over the whole abdomen, and in some of the gravest cases there is some general abdominal tenderness but no special local tenderness in the appendix region. As one other instance, persistent or increasing local tenderness may be almost the only sign of localised suppuration within or near the appendix—a condition the prognostic significance of which is obvious. (*c*) Muscular rigidity corresponds more or less closely with cutaneous hyperæsthesia and tenderness, so that in relation to prognosis the two would be noted together. (*d*) Local tumour or deep-seated resistance is very common, but as it is frequently absent in severe cases with diffuse peritonitis, and often not discernible at first, its prognostic value is very limited. It is important to bear in mind, contrary to what was believed in the old days when perityphlitis was held to be due to fæcal accumulation, that the absence of any tumour or swelling in the right iliac fossa not only does not contra-indicate the existence of perityphlitis, but may be of serious prognostic significance in relation to it, as being suggestive of the absence of the local peritonitic barrier, which so often shuts off the infected area from the general peritoneum. On the whole we are justified in saying that given appendicitis a definite local tumour is reassuring. What has been said as to the significance of local tumour would, with slight modification, apply to dulness on percussion.

4. *Progress of the Case.*—In many instances, when first seen, the condition has so far developed as to provide definite data for prognosis; if, however, the case is seen early this may not be so. Under such circumstances the development of events in the ensuing 24 hours, 36 hours, or 48 hours may furnish the needed indications for prognosis and treatment. In the milder cases symptoms will tend to recede, or at least to become more localised. On the other hand, if the affection be of a severe type the symptoms will persist or become intensified, though sometimes only after a treacherous remission which may very readily mislead.

2. Prognosis as to Recurrence.

Statistics vary as to the frequency of recurrence; some authorities place it as high as 47 per cent., others at below 30 per

cent.; in my series it was 31 per cent. It may, then, be expected in about one-third of the cases. We also know that the tendency to recurrence usually manifests itself within two years, and most commonly within one year of the first attack, although in some cases there may be an interval of many years between the first and the subsequent attacks. Besides such general facts to guide us the following clinical features will aid in forming an opinion as to the likelihood of recurrence in any particular case:—The nature of the first attack: If there were local suppuration the probability of recurrence is relatively slight; on the other hand, a mild first attack is frequently followed by recurrence. Convalescence from the first attack: If the condition clear up but slowly and local pain, local tenderness, or slight swelling persist for some time, the probability of relapse sooner or later is very great. Condition in the interval between attacks: If there is occasional pain or discomfort with irregularity of the bowels and definite local tenderness or resistance, a chronic appendicitis probably exists. The palpation of the enlarged and thickened appendix will render the latter diagnosis certain, which means a chronic tendency to relapse so long as the appendix is allowed to remain there.

Mr. Edmund Owen remarked that the best way to acquire practical information from a valuable paper, such as that just read, was by full discussion; and that the best way of starting a vigorous discussion was, not by merely praising the paper, but by keenly criticising it. In the first place then, he would ask why Dr. Caley gave statistics concerning cases of inflammation of the appendix which had been treated in the medical wards separate from those dealt with in the surgical wards, when, as was well known, the worst cases were often sent from the medical wards into the surgical wards for active treatment. He was strongly of opinion that every case of inflammation of the appendix coming into a hospital should be sent straightway into the surgical wards—though not necessarily for operation—and that, if medical aid were needed, the surgeon might be trusted forthwith to invite the help of his colleague. He was afraid that, if such a case went into a medical ward after the physician of the day had completed his work, there might be the loss of many valuable hours before surgical assistance was sought. He regretted that Dr. Caley had not confined his remarks upon the prognosis of appendicular inflammation entirely to the *clinical* aspect of the question, for no one could possibly tell the exact pathological condition of the appendix until it was actually exposed to view. There was always a great element of uncertainty as to what was the exact condition of the appendix, and as to how it was going to behave, and this uncertainty was the characteristic of the disease. When one was dealing with a case, say of renal abscess, cancerous bowel, or vesical calculus, one knew exactly what line of treatment should be taken, for each pathological condition ran, practically, a

definite clinical course ; but of the future of the inflamed appendix no one could speak with certainty, and generalisations drawn even from 2,000 clinical records would not be able to throw a clear light on any individual case for the treatment of which advice might be sought. He was glad that throughout his paper Dr. Caley had not referred to McBurney's point. Tenderness over McBurney's point was by no means a valuable clinical indication of inflammation of the appendix, and the practitioners who paid much attention to it would be very apt to be led astray. Tenderness over a spot a third of the way from the right anterior superior iliac spine towards the umbilicus might be due to many other causes than an inflamed appendix, and especially, as he had found in practice, to hyperæsthesia about the ovary. He agreed with Dr. Caley that one of the gravest signs as regards prognosis was the sudden onset of acute pain in the iliac fossa.

Dr. Fred. J. Smith regretted that the author had not discussed the question of prognosis more from a clinical point of view. His own object in dealing with such cases was to ascertain if pus were present or not ; if it were present there was then only one point in prognosis worth consideration, viz., the character and ability of the surgeon who was to operate. If one could be certain that pus was not present, or if there were doubt on the point, he believed that the administration of salicylate of soda and the local application of an ice-bag might be trusted for a few hours while the case was being carefully and constantly watched for any indication significant of suppuration. So strongly did his colleagues at the London Hospital hold the view that early operation was necessary to save life, that they were discussing the advisability of admitting all cases of appendicitis straight into the surgical wards.

Mr. Carless observed that it was in strict accordance with surgical principles to seek to anticipate suppuration, and not to wait for it to occur. If it were once allowed to progress beyond the walls of the process, diffuse peritonitis was likely to follow and might quickly run all over the abdomen. Early operation for the removal of the appendix would altogether eliminate the author's fifth class of cases, since nearly all of them were complications due to delay. Another reason why it was desirable to anticipate suppuration by early operation was that the appendix could then be removed entirely ; whereas, with a fully formed abscess, this was often impossible, and a subsequent operation for its removal became necessary. Yet another reason was that the formation of adhesions was thereby prevented, and thus the fourth class of cases would disappear. In fact, the surgical treatment of appendicitis within 36 or 48 hours of its onset, as practised by the Americans with such remarkable success, would probably be the treatment of the future.

Mr. Mansell Moullin said he thought 36 hours was the right time within which the surgeon ought to act. He really could not see why they were to wait for suppuration or diffuse peritonitis. Operation should be performed before suppuration set in. If a case of inflamed appendix did not improve within 36 hours the surgeon ought to be called in and, if he were not sent for, the responsibility would rest with those who neglected to do so. If the effusion continued to increase after the lapse of 36 hours it meant either that suppuration would set in, or that adhesions would form and render a relapse inevitable. The plan adopted by American and German surgeons of operating in all cases in which there was no improvement at the end of 36 hours, had given excellent results. He agreed that there was such a thing as gangrene of the appendix before

the onset of peritonitis, in fact, it might occur in less than 36 hours. He had seen a case of that kind in which the patient would have died of peritonitis had he not been operated upon within that limit of time. The whole question, with regard to treatment, was whether they could forestall suppuration and peritonitis ; and he was strongly of opinion that they could.

Mr. James Berry observed that the question before them was the advisability of the removal of the appendix at an early stage. He knew many surgeons who advocated its early removal in acute cases, but none of them, so far as he was aware, had brought forward any large and complete series with detailed statistics. He thought that perhaps it would be well if there were a little healthy opposition. Some few years ago he held the same views as some of the speakers had expressed. When, some seven years ago, he was appointed registrar at St. Bartholomew's Hospital, he had abundant opportunities of watching the effects of the practice of various surgeons, some of whom acted upon the principle of very early intervention, while others preferred to wait until adhesions had formed. Under these circumstances he soon began to change his opinion ; for although a certain number of cases were sent to the surgeon too late, in which life might perhaps have been saved by early operation, there was another side to the question, for early operation was sometimes undertaken in cases which would not, he thought, have come into the *post-mortem* room had they been treated on less drastic lines. He could tell of many cases, apparently parallel, treated in the two different ways ; one set by early and free operation, and the other by more expectant treatment. He soon became a warm advocate of the more expectant method of treatment. Referring to what had been said by a previous speaker, he said he knew of no series of 100 early operations for acute appendicitis, and he would be glad to be referred thereto. They all operated on chronic cases, in the interval between the attacks, without any deaths at all, but the acute cases were very different. He then quoted some figures which he had prepared from the records of St. Bartholomew's during the last three years of his registrarship and the year following (1895–6–7–8), which he thought might be of interest to compare with the author's figures. In this period there were 130 cases of operations for appendicitis in the surgical and medical wards. This number included all cases of operation for appendicitis of whatever kind, except a very small number of mere exploratory operations in which nothing definite was found, one case of intestinal strangulation, and one of old appendicitis with sinuses. There were 42 chronic cases without suppuration, treated in a quiet interval by removal of the appendix, all of which recovered. Then there were 14 chronic cases with abscess, of which one died. There were 74 acute cases with suppuration—the particularly interesting class—which were treated by one or other of two widely different methods, either by free incision into the general peritoneal cavity, or by a limited incision through adhesions only. In the latter the operation was not done so quickly, more time was taken, and eventually a small incision was made through the adhesions. Of the 74 acute cases, 33 were treated by free incision, with 26 deaths ; while 41 were treated by limited incision through adhesions, with only two deaths. Among the seven cases of recovery after apparently free incision, there were, however, at least two in which there was strong suspicion that the incision was really made into an area of suppuration localised to a small portion of the abdomen ; in another the abscess was very minute.

Having personally seen nearly all the cases in the first three years of the period mentioned, he could assure them that a large number of cases in both classes were precisely similar. In most of the 26 fatal cases the abdomen was washed out, and it was against this practice that he wished most strongly to protest. He maintained that the practice of washing out, which was not confined to any one hospital, was a most fatal practice, and he was unable to understand how surgeons could go on doing it year after year. Only a few days previously he had heard it urged that this was the proper treatment. He could not help thinking that some of these cases might perhaps have been saved had the surgeon been content with sponging. In examining these appendix cases, *post mortem*, he had drawn up a series of small diagrams of the area of abdominal wall through which an incision might have been made into the abscess, through adhesions only, and these he handed round. He protested once again against the practice of opening an appendicular abscess through an incision in the linea semilunaris. He called their attention to six cases in which they would see that the situation of the inflammation was such that, if an incision in the peritoneum had been made above a line drawn from the anterior superior spine to the umbilicus, it could not possibly have opened up the abscess without opening up healthy peritoneum. Lastly, as to McBurney's point, through which an incision had been recommended parallel with Poupart's ligament, he stated that not in one case would it have opened the abscess. He admitted that there were rare cases where the abscess formed near the umbilicus or in the loin, but these were quite exceptional. He recommended an oblique incision made about 1½ inches in length immediately above the outer half of Poupart's ligament, this being the incision adopted in the majority of the successful cases.

The PRESIDENT thought they should still continue to admit these cases into the medical wards. He had had a good many of them in his time (assuming the diagnosis to have been correct) which had recovered under common-sense treatment without having recourse to surgical interference. He did not, therefore, advocate handing them over immediately to the surgeon. At the same time, whenever he had a case of the kind he immediately got a surgeon in touch with it, and if he said an operation ought to be done, why, of course, it was done. Nevertheless, he did not think that cases in which symptoms pointed to the appendix should go straight to the surgical wards, for if some surgeons were fairly conservative, others were not. Mr. Carless had quoted American practice in favour of early operation, but he was under the impression that in America the appendix was often removed as a preventive measure, so that appendicitis could not occur. He was delighted to find that Mr. Berry was in favour of leaving something to the physician, whose sphere was nowadays becoming more and more restricted. He regretted that the subject had not been discussed more from the point of view of prognosis.

Dr. CALEY, in reply, said he had purposely avoided the question of treatment, his paper representing an attempt at a careful study of appendicitis with a view to prognosis. Even if they agreed that early incision was advisable, it was not such an easy matter to get the cases within 24 to 48 hours. There was a good deal to be said for the removal if they could get the cases early enough, and could be sure of operating at the best moment ; but he would ask whether it was not possible, by operating at a particular time, to sometimes convert what was strictly a local infection into a more generalised one. He protested that no physician would allow a case of appendicitis to be left in charge of his house

physician for 48 or even 24 hours, and it was a standing rule that such cases should be seen by the physician at once. Most of them would, in acute cases, act upon the principle of bringing the surgeon into touch with the case at once, in order to discuss whether and when to intervene. He pointed out that in both of Mr. Owen's cases there had been a pre-peritonitic stage, and if, when tenderness was complained of, the patients had been taken in hand, it was quite possible that the lesion would have been more localised. With regard to anticipating suppuration, he observed that if they could they ought to do so, and no physician would argue otherwise. That, however, was rather begging the question, and brought him back to the point he had already raised whether by operating at an injudicious moment they might not, under certain circumstances, do more harm than good. He regretted that so little notice had been taken of the points he had mentioned as guiding to a prognosis. With regard to the criticisms addressed to the pathological basis of his paper, he observed that it was impossible to consider the prognosis in any disease, unless what they knew of the clinical aspect accorded with definite pathological facts. He had hoped to elicit an expression of opinion as to the relative prognostic value or diagnostic value of the different symptoms. He believed that, after sufficiently careful examination, if the physician watched the case from hour to hour most cases could be differentiated.

December 11*th*, 1899.

ON THE SURGICAL TREATMENT OF THE ASCITES OF CIRRHOSIS BY THE ARTIFICIAL PRODUCTION OF PERITONEAL ADHESIONS.

By H. D. Rolleston, M.D. Cantab., F.R.C.P. Lond., and G. R. Turner, F.R.C.S. Eng.

In the following paper we propose to give a brief account of two cases treated according to the method introduced by Dr. D. Drummond and Mr. J. R. Morison,* and to offer some remarks on points of interest, especially on the technique of the operation and on the *rationale* of the treatment.

Case 1.—A man, aged 45 years, was admitted to St. George's Hospital on June 22nd, 1899, having vomited about three quarts of blood in the previous 48 hours. He had been accustomed to take three or four glasses of beer a day, had had syphilis 27 years ago, and had scars around the left knee. There had been no previous attacks of hæmatemesis. The spleen was readily felt to be enlarged; the liver was not enlarged, there was no evidence of gastric ulcer, and he was accordingly treated for hæmatemesis due to cirrhosis. During the next six weeks he remained

* 'Brit. Med. Jour.,' vol. ii, 1896, p. 728.

in bed with a raised temperature for which no cause except cirrhosis was forthcoming. His abdomen became distended with flatus, so that the spleen could no longer be felt ; about three weeks after admission there were œdema of the feet and ascites. Iodide of potassium was given but it in no way improved his condition, and, after consultation, it was decided, with the patient's full approval, to perform laparotomy and to set up artificial adhesions between the liver and the parietal peritoneum.

On the morning of July 31st the abdomen was tapped and sixteen pints of ascitic fluid were withdrawn. Mr. Turner opened the abdomen by an incision five inches long parallel to, and a finger's breadth below, the right costal margin. The peritoneum, although there was no peritonitis, was much injected. A large quantity of clear ascitic fluid was evacuated. The liver was markedly hobnailed but was free from adhesions. Its surface and that of the diaphragm were scraped with the finger, sponge, and director as far as could be reached on both sides of the suspensory ligament. A kangaroo tendon was then passed through the edge of the liver, the omentum, and the cut parietal peritoneum, and the three structures were tied together so as to bring the omentum up between the liver and the diaphragm. The peritoneum was then sewn up separately with the continuous catgut suture and the muscles were united by interrupted catgut and the skin by silkworm-gut and continuous horsehair stitches.

After the operation the patient steadily improved and left the hospital on August 27th. He now feels much better in health but still has enlargement of the spleen. He has been a little depressed in mind at times without, he thinks, adequate cause ; the only complaint is of occasional dragging, possibly due to the adhesions in the right hypochondrium. He has been recently seen (December 11th) and no longer complains of this symptom. In every way he is much improved and his splenic dulness is much less than formerly. There are, however, a trace of ascites, and some œdema of the feet.

Case 2.—A Frenchman, aged 52 years, was admitted to St. George's Hospital on June 21st, 1899, with ascites of two months' duration and œdema of the feet of two weeks' duration. For four months he had had morning vomiting and pain over the liver, and for two months the amount of urine had been scanty. Syphilis was denied, but he admitted to having taken whisky occasionally and large quantities of wine in Paris. The latter point was of interest in connection with Lancereaux's theory that cirrhosis is not due to alcoholic drinks, but to the sulphate of potash with which the *vin ordinaire* of Paris is plastered.* The patient's abdomen was greatly distended with ascitic fluid, and four days after admission 18 pints were drawn off ; 12 days later the abdomen was again quite full of fluid, and as the patient's general condition was deteriorating and his strength failing, the question of operation was raised and Mr. Turner agreed to operate.

On July 14th the abdomen was opened by a vertical incision in the right semilunar line. On opening the peritoneum a large quantity of turbid yellowish ascitic fluid escaped. The liver was markedly cirrhosed. Its upper surface and that of the neighbouring diaphragm were scrubbed with a sponge and scratched with the finger-nail and director. The margin of the liver was fixed to the abdominal wall by a kangaroo

* 'Bulletin de l'Académie de Médecine,' 1897–98.

tendon suture and the wound was united with silkworm gut passing through the whole thickness of the tissues.

The patient recovered well from the operation, but, 10 days after it, there was free fluid in the peritoneal cavity which increased so rapidly that two days later paracentesis to 16 pints was required. The fluid was turbid from the presence of numerous leucocytes ; there was no fat in the ascitic fluid, as was shown by extraction with ether. After this time (July 29th) the patient was tapped five times, and went out of the hospital on September 20th. When heard of in the middle of November the patient was ill in bed with a distended abdomen and œdema of the legs.

Results.

Of the two foregoing cases one of the patients improved, lost the ascites almost entirely, and felt much better in health, but was left with an enlarged spleen, so that he cannot as yet be regarded as cured. Talma,* in fact, under similar circumstances did a second laparotomy and sutured the spleen to the abdominal parietes, with the result that the organ diminished in size. In our other case the ascites recurred again and again, and though the patient's general state was much better than before the operation, when it appeared that life would not be much prolonged, the result can at best be regarded as only slight amelioration. One of Dr. Drummond's and Mr. Morison's patients lived for 19 months after the operation, having required 69 tappings.

In estimating the effects of the operation in such cases there is, perhaps, some danger in regarding the prolongation of ascitic life as due to the operation. It is true, as Dr. Hale White † has shown, that the subjects of pure cirrhosis seldom live to be tapped more than once or twice, but occasionally patients with ascites due to cirrhosis do recover, and the exceptional event must be reckoned with in considering the effects—especially when they are slight—of the operative treatment. It might, indeed, be suggested that the mere fact of opening the abdomen so alters the conditions there that, as in tuberculous peritonitis, this measure is sufficient to lead to a cure. But the abdomen has been opened a considerable number of times as a result of an erroneous diagnosis of ovarian cyst, &c., and when ascites and cirrhosis were discovered the abdomen has been closed. These cases, so far as we know, do not show any improvement, so that "abdominal

* 'Berliner Klinische Wochenschrift,' September 19th, 1898.

† 'Guy's Hospital Reports,' 1892.

section as a medical measure" does not appear to be of any use in hepatic cirrhosis without some further steps.

Of the previously published cases, ten in number,* four (Drummond and Morison, Morison, Talma, and Neumann†) of the patients have recovered, while of the remainder three at least died from the effects of the operation. The operation has to our knowledge been performed with a subsequent fatal result (the *post-mortem* examination showing an almost complete absence of adhesions), and it is possible that the published results are more favourable than the complete record of all cases thus treated would be.

Early Performance of Operation Desirable.

It is important that the operation should be done comparatively early and not postponed until the patient is too debilitated to withstand it, for patients with cirrhosis are far from good subjects, inasmuch as they are prone to secondary infections, especially to peritonitis. Thus of the ten cases published, two patients died from peritonitis and one from shock immediately after the laparotomy. Another reason for not delaying operative interference is the importance of intervening before the liver tissue is so degenerated that it is unable to undergo compensatory hyperplasia as the result of the improved blood-supply provided by the adhesions; this point will be referred to later. Medical measures should only be persisted in while the diagnosis is open to doubt, and they should be specially directed to counteract any possible syphilitic disease of the liver. When a course of iodide of potassium has not benefited a case of ascites which is thought to be due to either syphilis or cirrhosis, medical treatment should be suspended and operative measures employed.

When cirrhosis can be diagnosed with fair certainty in the pre-ascitic stage, and when there are evidences of the disease, such as hæmatemesis and splenic enlargement, operative interference would probably succeed better than in the late stages. Of our two cases the one operated upon earlier did the better. We are therefore inclined to believe that the early treatment of cirrhosis of the liver by operation has a future before it.

* Weir ('Medical Record,' February 4th, 1899) summarises the results of six previously published cases and adds one of his own.

† 'Deutsche Medicinische Wochenschrift,' June 29th, 1899.

Technique of Operation.

Of the two cases which we have recorded, the patient on whom an oblique incision skirting the costal margin was made, and whose omentum was stitched between the liver and diaphragm, did the better. The oblique incision would seem to give better access to the upper liver surface on both sides of the suspensory ligament and to enable the omentum to be stitched on that surface between it and the diaphragm better than does the one in the semilunar line, and it consequently is the one that we would recommend to be practised in the future for such cases. We would venture also to urge that such stitching of the omentum, the liver, and the abdominal parietes together is much more likely to be followed by the attainment of effective vascular adhesion than the mere stitching of the omentum to the abdominal parietes. The cirrhotic liver bleeds but little on passing the stitches, and we regard adhesion between it and the omentum to be most effectively obtained by this method.

With regard to suprapubic drainage, our opinion is that this is necessary in cases where there is a large amount of fluid present, and where there has been a rapid re-accumulation after previous tappings. It is quite possible that had this measure been practised in the second of the two cases the result might have been better. Both of the patients after the operation were kept as far as possible in the sitting position. And this, we would suggest, is obviously more advantageous than the recumbent one, as it must tend to allow of the more satisfactory formation of adhesion of the stitched structures in the upper abdomen.

With regard to the advisability of a secondary operation on the spleen, it may be interesting to note that the size of the spleen in Case 1 has markedly diminished since the operation, and this fact is perhaps an argument for at least delay before deciding to follow Talma's example. How far the splenic improvement is due to the operation on the liver must be uncertain. In the cases hitherto published, in one case the gall-bladder has been stitched to the abdominal parietes. The cases given here, however, would seem to be the first in which the stitches have been passed through the liver substance, the omentum, and the abdominal peritoneum.

Discussion of the Manner in which the Operation Improves the Condition of the Patients.

Does the improvement both in the general health and in the local condition—viz., the disappearance of ascites—simply depend on the collateral circulation relieving the pressure in the portal vein? This was the assumption on which Dr. Drummond and Mr. Morison planned their operation, and more recently it has been emphasised by Weir. Against this it may be argued (*a*) that ascites does not occur when the blood-pressure is presumably highest in the portal vein—viz., earlier in the course of the disease when hæmatemesis is met with; and (*b*) that ascites is a late manifestation and appears to be rather a result of a toxæmic condition of the blood than a mere mechanical result of increased portal blood-pressure, and that it is the outcome of a poison in the blood exerting a lymphagogue action.* The toxæmic state depends on hepatic insufficiency—in other words, on the cirrhotic liver being unable to destroy poisons that are continually passing to it from the alimentary canal; these poisons, therefore, reach the general circulation and lead to œdema of the feet, ascites, and to the constitutional nervous symptoms of the late stages of cirrhosis. If the collateral circulation between the peripheral parts of the portal vein and general systemic veins is markedly increased less blood will go through the liver and the toxæmia will be increased. In fact, the collateral circulation between the portal and the general venous system is carried to its logical conclusion in Eck's fistula. In this experiment the portal vein is interrupted in the portal fissure; its proximal end is closed while its distal end is put into communication with the inferior vena cava. In other words, the portal circulation through the liver is short-circuited, and all the blood from the intestinal area enters directly into the inferior vena cava, as shown by the very successful experiments of Hahn, Nasse, Nencki, and Pawlow.† In dogs this procedure tends to induce a disposition to uræmia. Thus if the dogs were fed on meat severe nervous disturbance, depression, asthenia, clonic and tonic spasms, and coma were brought on, and

* *Cf.* Hale White, 'Clinical Journal,' April 26th, 1899.

† 'Archiv. für experimentelle Pathologie und Pharmacologie,' Band xxxii, S. 161, 1893.

sometimes, indeed, ended in death. It is noticeable that one of Mr. Morison's* patients was alternately excited and depressed for three weeks after the operation; this was regarded as due to intestinal products passing directly into the general circulation without the intervention of the liver.

Thus, though an increase in the collateral channels between the portal vein and the systemic veins might modify the engorgement of the portal vein, it would not tend to improve the general state of health, but would have rather the opposite effect. Inasmuch, therefore, as ascites is not a mere mechanical result of increased pressure in the portal vein, and as a very extensive communication between the portal vein and the general venous system leads to a deterioration of the general health, it appears that the good effects of the collateral circulation are not solely due to relieving the pressure in the portal vein.

It seems to us that there are two possible ways in which the development of a collateral circulation in adhesions around the liver may be beneficial to the economy. 1. By somewhat diminishing the flow of blood through the liver it may enable that organ to deal more satisfactorily with the blood passing through it, and so reduce the toxæmic condition of the blood, which is probably the important factor in inducing ascites. 2. That the increased vascular supply to the surface of the liver may, by improving the nutrition of the hepatic cells, enable them to undergo compensatory hyperplasia. The compensatory hypertrophy of the liver† will enable the organ to perform more efficiently its important antitoxic functions, and so lead to a latency of the symptoms. If the last hypothesis be true, it is evidently of importance that any operation for the formation of vascular adhesions around the liver should be undertaken before the liver tissue is so disorganised that compensatory hyperplasia is impossible.

* 'Lancet,' vol. i, 1899, p. 1426.

† It is interesting to note that in a case of hepatic cirrhosis, in which it was suggested that the symptoms were arrested as the result of universal peritoneal adhesions, the liver weighed 89 ounces and presented nodules on its surface, which may be interpreted as due to compensatory hyperplasia of the liver cells. The patient died from poisoning by mussels. (F. P. Weber, 'St. Bartholomew's Hospital Reports,' vol. xxxiv, p. 321.)

Dr. Parkes Weber referred to the case described by him,* and mentioned by Dr. Rolleston, of cure of ascites due to cirrhosis of the liver. This case was not treated by the authors' method, in that there was no operative interference except by the usual paracentesis abdominis. This was performed twice, after which, although some fluid remained for a time in the peritoneum, the patient practically recovered. Dr. Weber raised the question whether there were any cases which should decidedly not be treated in the way adopted by the authors. No one, of course, would dream of treating the very bad cases of cirrhosis in the last stage by such a serious operation. In regard to other less advanced cases, the ascites might be tapped first of all in the usual way. If the ascitic fluid were of relatively high specific gravity, there was reason to suspect the presence of a rather more inflammatory condition in the peritoneal cavity than was usual in ascites. He believed, indeed, there was usually a slight amount of peritoneal inflammation in ascites connected with early "hypertrophic" stages of cirrhosis. If the specific gravity were as high as 1020, as in the particular case alluded to by Dr. Weber, it might be that one or two tappings would prove sufficient to bring about a cure (possibly with universal peritoneal adhesion), as it did in that instance.

Dr. Fred. J. Smith commented on the fact that the authors, in one case, had performed ordinary paracentesis on the morning of the operation, which he hardly thought justifiable. It was impossible to diagnose adhesions, &c., beforehand, and he thought the method was very unscientific Ought they not, he asked, to substitute a scientific laparotomy by a surgeon for paracentesis abdominis in every case? In regard to the treatment of cirrhosis by laparotomy, he said that within the last nine months he had had two cases operated upon for the relief of extreme ascites, both being cases of cirrhosis of the liver. In both cases the ascites had come on with comparative rapidity, *i.e.*, in about three months, and the fluid was very blood stained. The surgeon who passed his hand through the cavity said the liver was hob-nailed and rough. He wished to know what was the meaning of this bloody serum. One patient died within three days, but the other lived three months. He asked whether this would alter the authors' opinion as to the propriety of scraping the liver, whether, in fact, they would advise those measures if they found this blood serum? He personally would be inclined under such circumstances to close the abdomen without doing anything more than letting out the fluid. With regard to the treatment of the cirrhotic liver, he thought the presence of ascites, with cirrhosis, indicated that the patient's days were numbered; he would certainly look askance at any operative procedure other than skilfully opening the abdomen to relieve ascites which refused to disappear under medical treatment by drugs.

Mr. Bidwell said that he also had treated two cases of cirrhosis of the liver by operation, though both were unsuccessful. These cases had previously been treated by tapping; the first eight times, and the second three times. He had not sutured the liver to the abdominal wall, but had simply rubbed it with some gauze—possibly sutures would have been better. The first patient lived a fortnight; in the other case the fluid re-accumulated and was withdrawn by paracentesis two or three times subsequently, the patient ultimately dying from the disease. He thought the incision recommended by Mr. Turner in the first case was the best one. He was disinclined to adopt the suggestion to suture

* 'St. Bartholomew's Hospital Reports,' 1898, vol. xxxiv, p. 321.

the omentum between the liver and the abdominal wall, because he thought it might interfere with the formation of direct vascular adhesions between the liver and parietal peritoneum.

Mr. Wallis referred to the case of a woman, said to be alcoholic, whose abdomen was distended with fluid. He withdrew 22 pints, and she was then admitted to Charing Cross Hospital, where she was tapped and varying quantities of fluid taken from her, viz., 16, 12, 11, and 8 pints. That was four and a half years ago, and for the last three years she had remained in comparatively good health.

Dr. W. Ewart referred to two cases under his care which had been treated by this method. Particulars of one of these had been read before the Harveian Society. The patient recovered after severe and almost fatal peritonitis, but, as in one of Drummond and Morison's cases, the ascites recurred again and again, and, after upwards of 50 tappings, the patient died of exhaustion. On examination, the liver was found to be healthy and densely adherent to the diaphragm and abdominal wall as well as to the neighbouring viscera, which were all contained in the upper part of the abdominal cavity, the lower being occupied by fluid. The disease was cardiac, not hepatic; the heart was free from valvular affection, but was encased in an adherent and calcified pericardium. In Morison's case of long survival after the operation, with many tappings, the cause of death was not ascertained. Cases such as these indicate the need for some sure test identifying the presence of cirrhosis. This test might, perhaps, be supplied by an examination of the ascitic fluid withdrawn, as suggested by Dr. Parkes Weber. In the other case—one of the earliest laparotomies performed in imitation of the original operation, the details of which, however, were not all carried out—the liver was cirrhosed, but it failed to be adherent, in spite of the occurrence of peritonitis. The patient recovered from the operation, but ascites recurred, and he died after a tapping followed by irrigation of the peritoneum with weak solutions of iodine. In both cases Mr. Warrington Haward operated through a median incision and, in the first case narrated but not in the second, the liver was sutured to the abdominal wall and a counter opening made for suprapubic drainage. In spite of this want of success, Dr. Ewart was in favour of resorting to the operation in cases of well-ascertained cirrhosis. The practical question would be—how early in the course of the disease might it be recommended?

Mr. Turner thought it was important to make the incision as they had described, viz., parallel to the ribs, and he regarded stitching the liver and omentum to the peritoneum as a step to be decidedly recommended. In the first case, in which he simply rubbed the opposing surfaces, he was struck by the impossibility of any adhesions taking place after such handling. He thought it was a good thing to tuck up the omentum. In a recent case, in which there was obstruction of the common bileduct, the omentum was strongly adherent to the liver, and he had to separate it before he could get down to the gall-stones. The amount of vascularisation struck him as remarkable. Adhesions between the omentum and liver, which sometimes took place as the result of inflammation, were very dense. With regard to the paracentesis on the morning of the operation, he thought that the evacuation of such an abundance of fluid, which could not be done without anæsthesia, rendered the subsequent operation of less gravity than if he had attempted to do the two things at the same time In any

future case in which there was a large collection of fluid, especially if the patient's condition were in any way grave, he would certainly remove some of it beforehand, because such a measure shortened the subsequent operation.

The PRESIDENT said that he had long been interested in the treatment of ascites. Some of the speakers seemed to be afraid of tapping for this condition, but he himself had practised paracentesis with the greatest success in a number of cases. He had known some cases, even of ascites from heart disease, in which life had been prolonged for two or three years by this measure. He himself would not rush into an operation such as that now under discussion, but it might, under certain circumstances, unquestionably offer chances to the patient which he would not otherwise get. But before adopting an operation of this sort, he thought they ought to take every aspect of the question into careful consideration in relation to each individual case.

Dr. ROLLESTON, in reply, admitted that there were cases of ascites due to hepatic cirrhosis which recovered after tapping, but they were exceptional, and, as such, were apt to make an impression on one's mind and memory that was out of proportion to their frequency. Statistics showed that ascites in cirrhosis was a terminal phenomenon. If, therefore, an operation could be undertaken with reasonable hope of success in these cases, he contended that it was a right and proper course to adopt. Cases regarded as cirrhosis, in which numerous tappings were performed, were probably in reality examples either of chronic peritonitis or of syphilitic gummata involving the portal vein. With regard to the occurrence of hæmorrhagic effusions in the peritoneum in the course of cirrhosis raised by Dr. F. J. Smith—Barjon and Henry,* from critical examination of a number of cases, had come to the conclusion that it was due to traumatism exerted by the trocar, and that the blood came from the dilated vessels forming the collateral and compensating circulation in the sub-peritoneal tissues ; on the other hand, when a hæmorrhagic right pleural effusion was associated with cirrhosis, these authors showed that the cause of the blood in the effusion was tuberculosis.

THE CONDITION THREE YEARS AFTER OPERATION OF FIFTY SUCCESSFUL CONSECUTIVE OVARIOTOMIES : FIVE LOST SIGHT OF ; THIRTY-TWO IN EXCELLENT HEALTH ; SEVEN NOT QUITE SATISFACTORY ; THREE UNSATISFACTORY ; AND THREE DEAD.

(*Notes of the author's unpublished fatal cases of ovariotomy are added.*)

By JOHN D. MALCOLM, F.R.C.S. Edin.

THE statistics of ovariotomy have been made a subject of competition amongst surgeons, and it may be admitted that when the operation was new and its mortality was great, the

* 'Lyon Médical,' June 19th, 1898.

rivalry served a useful purpose; but now that the mortality is small, the announcement that one has attained the lowest death rate is apt to prove misleading; for the fact that a surgeon is able to publish a series of 50 consecutive recoveries from ovariotomy has not necessarily any scientific or clinical value. Even a much larger number of consecutive successes might be recorded without proving any special skill in the surgeon, whilst, on the other hand, there might be no discredit to an operator in the occurrence of fatal cases under his care at short intervals. The man who has the lowest death rate is not by any means necessarily the best surgeon. The mortality is influenced to a considerable extent by two factors which are quite independent of surgical skill. One of these is chance. A surgeon may have a long series of simple cases or of difficult cases, and this factor cannot be eliminated altogether in any number of operations.

It is obvious, however, that a man who has the opportunity of performing ovariotomy very frequently is likely to be asked to undertake an undue proportion of operations that are difficult and dangerous, so that his death rate might possibly be increased in consequence of his recognised ability. It is also obvious that if a surgeon selects a series of cases from his work, as I have selected the cases which are the subject of this paper, the results published may offer no indication of his true death rate. A brilliant series of successful cases may by chance be part of a very poor record.

Another factor which influences the death rate is the care and skill with which the cases for operation are selected. Some of the authors of statistical papers on ovariotomy have asserted that they made no selection of the cases operated on. I do not understand such an assertion. Every surgeon must select his cases. It is hardly possible that a surgeon could see a large number of patients with ovarian tumours without meeting with a malignant ovarian growth which he could not remove. If he has met with such a malignant case and has not operated on it, whilst asserting that he has made no selection of cases for operating, he really claims an infallibility of diagnosis which I have not seen at the bedside, and which, in fact, does not exist.

I have elsewhere pointed out* that a statistical paper on

* "Illustrations of Some Modes of Death after Ovariotomy," 'Med. Chir. Trans.,' vol. lxxviii.

ovariotomy has little or no value unless it is accompanied by an account of the cases simulating those of ovarian tumour in which the surgeon has declined to operate, but I do not know any series of cases in which this plan of publication has been adopted. In the series which I am now about to consider, there are three cases in which the mistake of refusing to operate was very nearly made, and in two of them operation was delayed a considerable time because of errors in diagnosis. In one I thought the tumour malignant, but on exploration of the abdomen I found a simple ovarian cystoma. In a second case, a soft fibroid had been diagnosed four years before I operated. In the third case, I opened the abdomen thinking that the patient suffered from chronic inflammation of the ovaries and Fallopian tubes. She had been under my observation as a hospital out-patient for many months, operation having been postponed from time to time after consultation with my colleagues. These cases might easily have passed out of my care, and, indeed, the second and third had passed out of other hands before I saw them. From this it is obvious that I may at times have seen cases of ovarian tumour without detecting the condition, and, of course, other surgeons may have had a similar experience.

Apart from cases in which the existence of an ovarian tumour is uncertain, there are cases, such as two of those recorded at the end of this paper, in which ovarian neoplasms are diagnosed, but it is doubtful whether the patient has any chance of recovering from the operation of ovariotomy. Under such circumstances surgeons may come to different conclusions as regards the advisability of interfering, some holding that an operation is useless and therefore should not be performed, others maintaining that the boldest surgery is justifiable, because cases of ovarian tumour, complicated by very serious exhaustion or other adverse conditions, are almost invariably fatal if left alone, whilst the patient may sometimes be restored to a long life if her tumour is successfully removed. Clearly the decision come to in such cases must influence the death rate, and therefore some record of cases in which ovarian tumours are known to exist or might possibly exist, but in which no operation has been performed, is essential if the publication of the statistics of ovariotomy is to have any scientific value, or any value for comparison with the statistics of other operators.

It is obvious that a man who is keen to exhibit a low death rate must tend more or less strongly to avoid operating on serious and dangerous cases. Quite apart, however, from any idea of future publication, every surgeon, I think, must have occasionally felt some hesitation in recommending and undertaking an unusually hazardous operation immediately after a similar case has proved fatal in his practice, or after a run of bad luck. That the treatment of a particular patient may be influenced by the state of a surgeon's statistics at the moment when he gives his opinion is a possibility which should be eliminated by every available means; but, undoubtedly, the practice of publishing and comparing statistics, without giving any details of similar cases in which operation has been refused, tends to intensify the influence of the surgeon's personal, even if temporary, feelings on his treatment.

There is another very important and serious objection to the practice of publishing and comparing statistics of ovariotomy, namely, the fact that the figures are not all drawn up according to the same plan. Some surgeons have set aside in supplementary lists those cases in which the operation is incomplete or otherwise complicated. I have never been able to see any justification for this method from a scientific point of view, and from a clinical point of view it seems to me simply misleading to exclude incomplete or complicated cases from statistical tables. By such means it is possible to modify the death rate to a very large extent. Take, for example, the six fatal cases I reported in the 'Medical and Chirurgical Society's Transactions,' vol. lxxviii. These were all my fatal cases of ovariotomy at the time of publication. Two of them were incomplete and two were complicated by hysterectomy; and for these reasons they might have been put in supplementary lists and excluded for purposes of quotation from my death rate. In the fifth case, the tumour had already been explored by abdominal section by one well-known surgeon, and another surgeon had refused to operate on it before I saw the patient. I completely removed a papillomatous tumour, and the patient survived 35 days, death being then due to obstruction of the bowels. I attributed this obstruction to the irritation of serous exudations and of frequent tappings which had induced extensive adhesions. There is no doubt in my mind that I did right in removing this tumour, and that both from a scientific and

from a clinical point of view the case should be counted amongst my fatal ones. But for purposes of comparison with other statistics, this case might fairly be set aside by me, or it should be carefully referred to by the other gentlemen concerned when their statistics are published. The tumour ought to have been removed when the abdomen was explored 18 months before I operated, and the chances of success would then have been far greater. Thus, for purposes of quotation, five out of six deaths might have been removed from my death rate. The sixth death was due to a uterine condition, and it also might have been excluded from my statistics without much ingenuity, leaving no mortality. Some such manipulation would have been necessary to get a fair comparison with many of the published tables, but all six cases died after ovariotomy or attempted ovariotomy, and I repeat that I have never been able to understand why each one should not count as a death in my statistics. Any other plan would amount to this, that in publishing statistics a surgeon may determine that certain cases in which he has removed an ovarian tumour, or part of one, are not to be considered cases of ovariotomy, and, if the patients die, are not to be counted as fatalities in his statistics of this operation, he alone being the judge as to which are to be eliminated. This method of setting aside and excluding from the death rate cases in which ovarian tumours have not been completely removed has, unfortunately as I think, received the support of men of very high standing; but if cases in which the surgeon has failed to complete his operation may be excluded from statistics, another surgeon might with equal reason exercise his ingenuity in proving that his cases died of complications and not of ovariotomy. Thus the statistics of this operation might eventually depend, and do now to some extent depend, on a skill and ingenuity which are certainly not surgical.

It is not always easy to say whether a patient's death is caused by an operation or not. For instance, a patient may get a pneumonia and die a week or ten days after a simple ovariotomy in which otherwise everything has gone well. Perhaps no one is in fault, and it may be abundantly clear that the death is not attributable to any want of skill and care on the part of the operator. It would be hard on a surgeon who was collecting statistics for purposes of comparison to have to include such a

case amongst his deaths. Nevertheless, in the circumstances suggested it is fairly certain that the patient would have escaped the pneumonia if the operation had not been performed, and although it might possibly be argued that a death from an intercurrent disease should be excluded from the surgeon's statistics, because there is no necessary connection between the operation and the death, it is also quite certain that in excluding such cases when quoting a death rate to a patient the surgeon would conceal a risk which the patient must undergo if ovariotomy is performed.

Again, it is sometimes not possible to say whether an operation is an ovariotomy in the sense of *removing an ovary for new growth* or not. At the end of this paper I shall refer to a fatal case in which I am not prepared to say definitely whether I removed a suppurating ovarian tumour or an abscess of the ovary. In publishing this case a surgeon might fairly choose to put it amongst his operations for new growth, or amongst those for inflammatory disease; but for scientific and clinical purposes, it would be better to refer to it in the statistics of both these operations.

A fatal case which I shall hereafter record illustrates a fallacy seldom noted in statistics. The case was first diagnosed as an ovarian tumour, but the disease was really a severe pelvic inflammation. When an ovarian tumour is diagnosed the possibility that the disease may be something else always exists. There are some conditions, such as those found in certain cases of retro-peritoneal cyst, and in some cases of cystic uterine fibroids, which it is quite impossible to differentiate from ovarian tumours before the abdomen is opened. Thus the risks run by a woman who is advised to have an ovarian tumour removed include those which may arise in case some other condition is found. Some of these conditions are easily dealt with by an experienced surgeon, so that possibly the average risks may not be greatly, if at all, increased by this possibility of a mistake in diagnosis being made. In one instance, however, that subsequently came under my care, a cystic fibroid tumour of the uterus had been cut down on, and the scar of the incision was $14\frac{3}{4}$ inches long when I measured it. A tumour of this kind always grows continuously, and therefore ought to be removed, but the operator neither removed it nor told the patient to seek further advice,

and an interval of two years and eight months elapsed before she came under my care. I performed successfully a hysterectomy, which was really a very simple operation. The case shows that the risks run by the patient, if a mistake in diagnosis is made, may be very considerable.

Owing to such considerations as the foregoing I have never paid much attention to statistics either in my work or in publishing an account of it. The majority of statistical papers concerning ovariotomy, especially those of recent date, seem designed chiefly to show the best side of a series of cases, and, if possible, to beat the record as regards recoveries. The fatal cases which occur in every practice, if it be long enough, are, however, the really instructive ones. I have therefore published most of the deaths from ovariotomy, or attempted ovariotomy, which have occurred in my practice, and I propose to relate those not yet published at the end of this paper. My object in the present communication is to record the condition of a series of patients three years after the last operation of the series was performed (October 2nd, 1896), and I have chosen 50 successful consecutive cases because they seemed to give a better foundation for a paper than a series in which there were some deaths from operation. I shall say little about the simple and satisfactory cases, but I shall record certain accidents and mistakes that occurred, and I will give a more detailed account of those cases in which the later condition is unsatisfactory, or not so satisfactory as might be desired. Deductions from a comparison with the statistics of others would be utterly illogical, for it is practically impossible that another series of 50 cases could occur in which the initial conditions would make such a comparison useful in any way.

Details of the operations and of the convalescence of the patients will be referred to only in so far as they seem important in connection with the more recent conditions to be recorded.

Four Cases refused Operation during the period covered by the Fifty Cases under consideration.

Before discussing the 50 cases referred to in the title of my paper, I propose to fulfil the conditions I have laid down as necessary in a statistical paper, by recording shortly the cases which I saw during the period covered by the series under

consideration, and concerning which I was conscious that I might possibly have made a mistake in refusing to remove ovarian tumours. These were four in number.

The first was the case of a hospital out-patient whom I was asked to examine by my colleague, Mr. Corrie Keep. She was an elderly woman, who had an abdominal swelling which I diagnosed as a malignant ovarian tumour, which I could not successfully remove. The inguinal glands were enlarged, and the tumour was firmly fixed in the pelvis. As the bowels were much loaded, I advised that the patient should take some laxative medicine, and be examined again later. She did not come back to the hospital, and I have no knowledge of her subsequent history.

The second case was sent to me by the late Dr. England, of Dover. The patient was a woman, 47 years of age, from whom Dr. Pregaldino, of Ghent, removed an adeno-carcinoma of the right ovary in November, 1894. On October 8th, 1895, she consulted me, and I found a tumour partly solid and partly cystic, which projected freely into the vagina to the left of the cervix uteri. The tumour extended along the left side of the uterus above the pelvic brim. The uterus was easily defined beside it, although firmly fixed to it, and the growth was so closely adherent to the pelvic wall that I thought any attempt to remove it would end in failure. I therefore advised that no operation should be performed, and the patient died nine months after I saw her. This is the only case I have seen in which a tumour that I believed to originate in the ovary sprouted freely into the vagina.

The third case was one in which I opened the abdomen on January 14th, 1895. I found a tumour of the left ovary, which was extensively adherent, but seemed as if it might be removed without much difficulty. Another tumour on the right side was firmly adherent to many coils of small intestine. The most easily reached of these coils had new growth penetrating its wall and protruding into the lumen of the gut. I did not persevere with the operation because a resection of bowel would have been necessary, and there were many adherent coils behind, any or all of which might have required similar treatment. The patient recovered, but I regret that I have lost all trace of her.

In the fourth case, in which I did not operate, an abdominal

and a uterine growth were discovered in a patient 50 years of age. The patient had a great deal of offensive discharge from the vagina, and the uterine cavity was dilated and curetted in order to determine the nature of the uterine disease. A malignant uterine growth was diagnosed, but the case was not considered suitable for hysterectomy. After the exploration there was an irregular temperature, with abdominal distension and pain. The gentleman who had curetted the uterus thought the patient had an ovarian tumour with a twisted pedicle, in addition to the uterine disease, and he asked and urged me to open the abdomen. It seemed to me, however, that if the uterine condition were malignant, the abdominal tumour was also malignant, and that the patient was suffering from an inflammation spreading to the peritoneum, from the recently disturbed intra-uterine diseased tissues. Under the circumstances I considered operative treatment quite hopeless. I therefore declined to interfere, and the patient died.

At the *post-mortem* examination a ruptured ovarian cyst was found, with acute inflammation of the peritoneum in its neighbourhood. There was no twist of the pedicle. There was no malignant disease; but the uterus was affected by fibroid neoplasms, some of which had sloughed.

The two questions of diagnosis which arose in this case are occasionally a source of very great difficulty. I was not in any way responsible for the diagnosis of malignant uterine disease; but some 12 or 13 years ago I saw a somewhat similar case, in which I did not know what the patient was suffering from. I consulted a man of far greater experience than my own, and he said without hesitation that the patient had a malignant growth of the uterus, a diagnosis which I accepted. A few days after our consultation I was sent for, because the patient could not pass water and was in great pain. She was in bed, and I suggested that she should try to pass water in the erect position. The bladder acted without difficulty, and the patient at the same time expelled from the vagina a putrid fibroid tumour of oval shape, measuring nearly 5 inches in its greatest length. After that she rapidly recovered her health.

In cases of doubt between malignant disease in the abdomen and an ovarian tumour, either on account of twisting of the pedicle or of rupture of one or more cysts, it is only by opening

the abdomen that it is possible to make the diagnosis certain. But if this question should be placed before me again in a patient whom I believed to be suffering from malignant disease of the uterus, with peritonitis developing soon after the interior of the uterus had been disturbed by exploration, I doubt if I would operate unless it seemed feasible to remove the uterus also.

Five Cases Lost Sight of.

As regards the 50 cases now under consideration, I regret that there are five of them whom I have been unable to trace. Their histories are, shortly, as follows:—

In one the patient was a married woman, 40 years of age. She had noticed a tumour for nearly two years. I removed a multilocular ovarian cystoma which distended the abdomen as high as the costal margin. There were some very vascular adhesions to the pelvis and bladder, but the highest temperature during convalescence was 100·6° F. in the vagina, and after three days the temperature was normal. The patient left the hospital on the twenty-third day after the operation.

In the second case the patient was a widow, 60 years old, and she had a large cystic tumour filling the abdomen. The tumour was deeply buried in the left broad ligament, and the sigmoid flexure was pushed high up in the abdomen by the tumour, to which it was firmly attached. There were some omental adhesions, but the chief difficulty lay in the deep enucleation from the broad ligament. The Fallopian tube formed a loop 6 inches long on the front of the growth, the fimbriated extremity being within an inch of the other end of the tube. The uterus was atrophied, and I could not discover the right ovary.

The highest temperature during convalescence was 100·8° F. in the vagina, and the patient left the hospital on the twentieth day after the operation, being then very well.

The third case was that of a neurotic single woman, aged 33, who had a very tender tumour the size of a duck's egg on the right side of the uterus. The pain made her quite unfit for work during four days of each period. At the operation there was no difficulty, except that the ovaries had very short pedicles. There was a small fibroid tumour on the right side of the fundus of the uterus. Both ovaries were removed, the right was cystic, and the

left contained a dermoid cyst about three-quarters of an inch in diameter.

During the first week of convalescence the temperature once rose as high as 101° F. in the vagina, the pulse at the same time being 70. The temperature came well down to normal; but on the tenth day there was a sharp rise to 102·2° F. Apparently this thermal disturbance was connected with the administration of a purgative. At this time the pulse did not rise above 72, and the temperature was again normal two days later. The patient left the hospital on the twenty-first day after the operation, and she was then in very good condition.

In the fourth of these cases the patient was 26 years old. I diagnosed and removed an extra-uterine pregnancy, the size of a large Tangerine orange. An ovarian tumour of somewhat smaller size was also removed. The highest temperature during convalescence was 101° F. in the vagina. The patient left the hospital on the twenty-first day after the operation, the temperature then having been normal for a week.

The fifth case was that of a married woman, aged 27 years. The patient had been told she was pregnant, although her menses had not ceased, and there was no enlargement of the mammæ, whilst the abdomen was as large as if it contained a uterus at the seventh month of gestation. The patient was very feeble, with a weak pulse and a loud hæmic murmur heard all over the cardiac area. She was much afraid of the operation. I removed over three pints of ascitic fluid and a solid ovarian tumour, weighing 2 lbs. 14 oz. Mr. Targett examined the growth, and informed me that it showed no evidence of malignancy.

This patient had an attack of bronchitis and a high temperature for nearly a week after the operation. On two nights the temperature was as high as 103·4° F. in the vagina, and the patient's condition made me very anxious, but she slowly regained her strength, and about 18 months later she was very well. Since that time I have not been able to trace her.

In the remaining 45 cases, 32 of the patients seem to me to be in really good health, seven others have very little to complain of, three are in an unsatisfactory condition, and three are dead.

Thirty-two Patients in really Good Health.

In 1887 I argued that the recovery of a patient after an abdominal section depends to a very great extent on the success with which the surgeon prevents intestinal troubles, and I showed that intestinal difficulties due to paralysis of the bowel and obstruction of the bowel had been generally mistaken for inflammatory conditions of the peritoneum. The views I then published are now universally regarded as correct, and the importance of paresis of the bowel as a factor in bringing about symptoms of obstruction after an abdominal section is insisted on by all who write on this subject. I would on this occasion further point out that not only the immediate result of the operation, but also the future health of the patient, is largely influenced by the extent to which the functions of the intestines are unimpaired, or are interfered with by peritoneal adhesions. If adhesions exist in such positions that the propelling effect of peristalsis is hampered in any way, they may induce much pain, constant indigestion, and a permanent liability to symptoms of intestinal obstruction of an acute or chronic character. It is an important fact that intestinal adhesions may stretch or become absorbed so as to give less trouble as time goes on. To such changes I attribute the fact that a number of the patients whom I now report as in every way satisfactory complained of bowel difficulties for varying periods after their operations.

It is not safe, however, to count too confidently on the disappearance of adhesions. I have evidence that they may be persistent many years after an operation, and there can be no doubt that they may continue indefinitely. Hence, if an acute illness, with intestinal difficulties that might be due to an obstruction by band, arises at any time after an abdominal section has been performed, the fact that these symptoms may be due to adhesions should be kept prominently in mind. Under such circumstances I would urge that all the arguments in favour of immediate operation should be considered of unusual weight. I have been forced in consultation to delay operation in two cases of this kind. In both, death was proved to have been brought about by an obstruction caused by an adhesion. Neither of the patients had the slightest chance of recovering without an early operation, and one of them would almost certainly have

recovered if she had been operated on when I first advised interference.

It is to be expected, therefore, that the best results after ovariotomy will be obtained in those cases in which the tumours have no adhesions and require no enucleation from the broad ligaments. If I put on one side the five cases lost sight of and the three that have died, I find that in 15 of the remaining 42 cases there were no adhesions, and no enucleation of the tumour was required. In 12 of these cases the present condition of the patients is thoroughly satisfactory. In 27 cases the operative difficulties varied from the slightest adhesion to complete union of the tumour to all neighbouring structures, and in some cases a very deep enucleation from the broad ligament was necessary. Of these 27 patients, 20 are quite well. Thus in this series the proportion of really satisfactory cases did not greatly differ, whether there were adhesions or not before the operation; but the three cases which are not quite satisfactory after simple operations have little to complain of, and might very possibly have been quite as uncomfortable if they never had developed tumours requiring removal by operation; whereas in the seven not quite satisfactory cases in which adhesions were found, three are now not at all as I would wish them to be.

Numerous adhesions do not, however, always lead to bad results. Adhesions cause no harm when they do not interfere with the function of the bowel. For example, the intestines in cases of tubercular disease may be almost or quite universally adherent, and yet bowel symptoms are not necessarily prominent. One of my cases in which adhesions must be numerous has already been published.* The tumour was an unusually adherent dermoid cyst, and the patient during convalescence gave no cause for anxiety for 16 days. She was then allowed to get up, and symptoms pointing to the existence of a pyonephrosis gradually developed; the patient became very ill, the temperature rose to 105° F., and she appeared to be dying. On the twenty-seventh day after the ovariotomy I incised the left kidney, which was very much enlarged, but I found no pus. Nevertheless, this second operation was followed by an immediate improvement, and convalescence progressed favourably, being interrupted only by

* Twenty-six cases in which an abdominal section has been performed a second time ('Med. Soc. Trans.,' vol. xx, p. 26).

occasional attacks of pain and high temperature, which, however, were always relieved by purgation. The loin wound closed, and in July, 1897, 13 months after the operation, I reported that the patient's health was thoroughly re-established. She still has occasionally a little pain, which is relieved by the administration of laxatives. She can walk as well as most of her friends, but she has not ventured to bicycle or take horse exercise, to which she had been accustomed. [Since the above was written the patient has told me that she is in excellent health.]

Within three weeks of the operation last referred to I removed another dermoid tumour from universal adhesions. These two operations were very similar, the patients being 32 and 35 years of age respectively. The second patient has also had occasional pains, which have been relieved by rest and laxatives, and she can now do everything she wants to, with the exception that if she stands for any length of time she becomes very tired. She can, however, bicycle and skate freely without the slightest trouble. In both these cases it is fairly certain that a great deal of adhesion of the intestines still exists, the tumours having been literally peeled out of strong inflammatory attachments. Both growths were very hard, heavy, and irregular in outline, with many bony parts projecting under the surface, and the adhesions seemed to be due to the mechanical irritation caused by the rough surfaces, for one tumour had no twist of its pedicle, and the other had only the slightest turn, which was not sufficient to obstruct its blood supply. Of course, it is impossible to say positively that these patients have any adhesions now, but recently I have performed second operations on two very similar cases, with intervals of 18 months and four years and three months respectively after the first operations, and in each of them adhesions were very numerous, firm, and extensive, around the site of the first operation.

Patients with such extensive adhesions, if they recover well from the operation, are more free from danger of serious trouble than they would be with one or two isolated bands of adhesion, because the danger of an obstruction arising after convalescence is well established is much less when a large area of intestine is fixed than it is when the intestines are for the most part freely movable and are held firmly at one or two points only.

The history of another patient in whom no adhesions were

found at the operation, shows that bowel difficulties do not always depend on adhesions, but may be caused by a sluggish peristaltic action, such as may be found in patients who have not been operated on. This woman was 36 years old and had suffered from severe constipation for many years, for which horse exercise had proved very beneficial. This kind of exercise had to be discontinued when the patient's abdomen became of great size from the development of two ovarian tumours. For four months before the operation the constipation was very obstinate; and during convalescence the patient was much troubled in this way until I allowed her to resume horse exercise, when the bowels acted more satisfactorily, and she very soon reported herself as quite well.

Some of the other cases that have made good recoveries have points of interest. In one I opened the abdomen, letting out much fluid, which I thought was free, in the peritoneal cavity. I tapped a very soft, friable, multilocular cyst, and then found that I was working within a thin-walled cyst and that I had tapped a secondary cystic growth. I was not able to separate the main cyst from the peritoneum of the anterior abdominal wall at the site of my incision. I therefore cautiously opened the cyst behind and exposed the small intestines, which were only slightly adherent to the tumour. I was then able to separate the cyst from behind forwards. It was very thin walled, and firmly adherent to the liver and to the peritoneum of the anterior abdominal parietes. The patient, who was 23 years of age, had an extreme lateral spinal curvature, and was very delicate. She had worn a spinal support from childhood. The enlargement of the abdomen caused by the tumour had been noticed only six months before the patient was sent to me.

This operation was a very dangerous one, on account of the extensive tearing of tissue that was necessary and the feebleness of the patient. So far as the operation is concerned, however, the result is perfect, and since she left my care the patient has been able to dispense with her spinal support.

In four of the cases, an ovarian tumour had already been removed at an earlier date. In one of these, the bowel was injured at the second operation; but the result is very satisfactory. Mr. Knowsley Thornton removed a tumour of the left ovary in 1882, and on November 18th, 1895, I removed a tumour of the right ovary the size of a small orange. This was deeply

embedded in the right broad ligament, with coils of small intestine firmly adherent to its posterior and upper surfaces. In order to make sure that the intestines were not punctured in removing the tumour, I had to separate a large mass of matted small intestine which was firmly adherent in the pouch of Douglas. The walls of the intestine were torn in many places, and at one point its lumen was opened. The perforation and many other injuries of the parts were sewn over with Lembert's sutures; the patient made a good recovery, and is now, four years later, quite well. As a rule, it is not wise to meddle with extensive adhesions which have caused no symptoms of intestinal obstruction, but sometimes, as in this case, the surgeon has to effect a partial separation, and then it may be wiser to proceed to divide all the adhesions. There is no more difficult question than to decide how much should be done in this way.

In one of the series, owing to a mistake on the part of a nurse in counting the sponges, I left one in a patient's abdomen and removed it half an hour after she was put to bed. She also is quite well and the incident of the sponge gave rise to no trouble.

On one occasion I removed a cystic tumour of the left ovary and found a small horn-like growth on the right ovary. As the disease on the right side seemed to be confined to a small portion of the gland, I removed the new growth with a wedge-shaped portion of healthy tissue and sewed the parts together by means of fine silk, leaving the greater part of the ovary. This patient writes that she has been in good health ever since she left my care.

The following case was reported as untraced when this paper was read:—The patient was a single woman, 23 years old, and I diagnosed an ovarian tumour deeply buried in the right broad ligament. On opening the abdomen I found, as I thought, a tumour closely attached to the right side of the uterus. The uterus seemed to stand out so well to the left of the tumour that I expected the latter when tapped to shell out of its capsule easily, and I therefore tried to complete the operation through a short incision. After tapping, however, I could not quite understand the conditions, and on enlarging the incision in the abdominal wall I found that I had tapped the pregnant half of a double uterus. I very carefully closed the punctured opening. There happened to be a small cyst of the right ovary, which I removed, and the existence of which brought the case within the series

under consideration. The patient of course miscarried, and I had to scrape away the placenta and membranes; but she made a good recovery. I lost all trace of her until February, 1900, nearly four years after her operation, when I heard that she had been delivered of a 7½ months' child in Queen Charlotte's Hospital. Dr. Levison, the resident medical officer of that institution, has kindly informed me that when she left the hospital the whole uterus was larger than normal, and that at the fundus it was possible to distinguish the left uterus, which was softer, from the right, which was smaller and firmer. The septum of the cervix was easily felt, the left os uteri being the more patulous. The patient was still a single woman. (Mr. Corrie Keep informs me that this patient, who had been under his care before I saw her, had been pregnant on a previous occasion—a fact which I did not know, or had forgotten.)

There is one other case that should be mentioned amongst those in which the results are quite satisfactory. The patient had a dermoid tumour of very irregular shape, which was deeply embedded in the right broad ligament and which was diagnosed by myself and others as a fibroid uterine growth. Before and for months after the operation, I found granular and epithelial casts in the urine, which was pale and was passed in large quantity. Its specific gravity was never above 1010, and it did not contain albumen. Five months after the operation the patient had an acute attack of nephritis, with much blood in the urine, but she is now in a very satisfactory condition; she looks well, does a great deal of work as a lodging-house keeper, and has not required medical assistance for nearly three years. A specimen of her urine examined in June, 1900, contained a decided trace of albumen and a very small quantity of pus or proliferated epithelial cells, with a few vesical epithelial cells, but no renal casts were found nor any blood. The urine was acid and its specific gravity was 1013.

Several of the patients have had intestinal difficulties from which they have perfectly recovered. In one, I thought a malignant recurrence might be developing, but the suspicious lumps which I felt in the abdomen were cleared away by laxatives and enemata, and the patient is now quite well again. In another case I removed a large multilocular tumour with very friable walls, which had already burst in places before the abdomen was

opened. The second ovary was also taken away. I thought both ovaries were completely removed, but the patient, whose age was 36, had a slight white and red monthly discharge from the vagina until very recently. Two years after the operation this patient had an attack of severe dragging pain in the abdomen, lasting a week, and she was afterwards confined to bed for four months, there being much difficulty in getting the bowels to act. The patient is now very well, and the periods have quite ceased.

In one case I thought that the tumour was malignant, but it was not examined microscopically. The patient has remained quite well. Several of the patients have borne children since their operations, one as early as 13 months afterwards.

Two patients have married after single ovariotomy, and two after double ovariotomy. The latter appear to be quite happy. (One of the patients married, after double ovariotomy, in July, 1899, at the age of 25. She was quite well till Christmas of that year, when, after nursing her husband through a serious illness of some months' duration, she suffered from bearing-down pains with a vaginal discharge, and became very thin. I have not seen her since then.)

In the whole series I have no knowledge of any case of hernia, but I have not examined all the patients.

Seven Cases not quite Satisfactory.

Of the cases in which there were no adhesions, and in which the results are not quite satisfactory, one was operated on when she was four months pregnant. The operation was quite a simple one, the tumour being of oval shape, and about 5 inches in its longest diameter. The patient had no trouble after delivery at term, but she took little care of herself. Although advised to wear a belt, she did not do so. She has now very lax abdominal walls, with wide separation of the recti muscles, but there is no hernia of the scar. The condition might have arisen without the operation having been performed, and was due to her neglect of obvious precautions. Of course the tumour described did not cause any very great abdominal distension at the fourth month of pregnancy. (This patient died of acute pneumonia on April 9th, 1900. Obviously the condition of the abdominal walls must have diminished her chances of recovery from an acute lung disease.)

The second case is that of a woman, aged 50, who had borne 10 children. She complains much of debility and of an occasional dragging pain in the side. She also has a "pendulous belly," for which she has not worn any support. When I examined these cases a few weeks ago I could not find anything abnormal in either of them, beyond the laxity of the abdominal walls.

The third patient was 38 years of age. She says she is in very good health, but suffers from occasional soreness in the left side. I have not seen her recently.

In these three cases, the unsatisfactory conditions might have arisen if the patients had never developed tumours requiring operations.

Nor are the conditions very serious in four of the seven cases in which, after operations complicated by adhesions, the results are not altogether satisfactory. In one of these cases I removed a small tumour deeply embedded in the right broad ligament. The patient was 37 years of age, and had complained of pain and weakness in the back for 16 years before the operation. Now, her only trouble is that her back is not strong. Everything else is satisfactory.

Another case is that of a woman, 43 years of age, from whom I removed an ovarian tumour, deeply embedded in the right broad ligament, and so thick-walled and red in colour that I mistook it for a fibroid uterine growth. Even after the abdomen was opened the tumour seemed to be a part of the uterus, but an exploring needle showed that it was cystic, and when the fluid contents were drawn off the relations of the parts were easily defined. The attachments were so extensive, and the tumour was so friable, that it was impossible to be certain that I had removed the whole of the new growth. The left ovary, which was also cystic and deeply buried in the broad ligament, was removed. This patient has complained much of pelvic and abdominal pains, but she can get about freely. She is very fat, but not flabby, and she has all the appearance of robust health.

Another patient, aged 23 at the date of her operation in January, 1896, had two ovarian tumours, each about the size of a big cocoa-nut. That of the left ovary, which had a very long pedicle, was situated in the right loin, no part of it being on its own side of the middle line of the body. The pedicle had become twisted, and the tumour was firmly adherent in the situation described. The

tumour of the right ovary was in the right side of the lower abdomen, below and rather to the inner side of the left one. It was firmly adherent to the other, and slightly adherent to neighbouring structures. Both tumours were removed. Since the operation the patient has been without any obvious sign of ill-health. She is stout and full blooded, but she has complained a great deal of pain in the back and sensations of creeping in the head, which are very distressing. The abdomen is soft, and natural in every way to palpation, but the bowels are very constipated. The case seems to be one of severe neurotic symptoms due to the artificial change of life, and probably a gradual improvement will take place. I may add that the patient's medical attendant wrote recently that she had enjoyed excellent health since the operation. She is now at work as a governess.

Another patient, a married woman, aged 34, suffered from chronic inflammation of the ovaries, accompanied by increasingly painful menstruation for seven years. She was under my observation for two years, and had been confined to bed for considerable periods under medical observation with only temporary benefit. The presence of new growth was not suspected. Eventually I opened the abdomen, and removed two small ovarian tumours which were deeply imbedded in firm adhesions. This patient made a rapid recovery, but her periods have not ceased. About 18 months after the operation they became somewhat painful, and there was tenderness in the left broad ligament on palpation. In this case the sexual appetite has been so increased as to be a source of great distress. When I operated I thought that I removed both ovaries completely, but it is not possible to be certain of this when many adhesions have been separated. [Since the above was written, this patient has had prolonged intervals between the periods, and probably the menopause will shortly be established. With the exception of the nervous symptoms, the general health is very good.]

The results cannot be described as very bad in any of these seven cases, although they are not so good as might be desired.

Three Cases Unsatisfactory.

In the three following cases the results, so far, have been decidedly unsatisfactory. In the first I removed a small flat

tumour of the right ovary from numerous very strong adhesions. The patient had felt big in the abdomen, and had suffered from increasing backache and difficulty in walking for three years before the operation. Since the operation she has had much trouble from constipation, weakness, and pain in the side aggravated by walking. Her condition recently in these respects is better than it has been, and I may add that the patient seems to consider her health much more satisfactory than I think it. [Since the above was written this patient has improved very much. She says she is in good health, though she cannot walk very far. She is able, however, to "do a great deal of walking about indoors, mounting the stairs (about 40) many times a day." She assists her brother in the management of a hotel. The bowels gave little trouble, and aperients are not required so often nor in such quantity as was necessary a year ago.]

The second unsatisfactory case was that of a woman who had suffered from attacks of pelvic inflammation for six years before I saw her. She had been in several hospitals. In one she had been put under an anæsthetic, and plugs had been inserted every night for a time, but she got no benefit. I removed a small cystoma of the left ovary which was firmly adherent deep in the pelvis. The ovary and Fallopian tube on the right side were also fixed by strong adhesions, and were removed with difficulty. Possibly the appendages were not completely extirpated in this case. The patient continues to have menstrual discharges of blood from the uterus, sometimes as frequently as every fortnight. She has had attacks of peritonitis occasionally, like those which occurred before I operated, and recently these attacks have been more frequent.

(In June, 1900, this patient was better in many ways, but she still had attacks of pelvic pain, although these did not occur so frequently as they did a year earlier. There was a tender thickening of the tissues in Douglas's pouch. The patient looked much more healthy than she did when I saw her 18 months earlier. These two patients are certainly improving in the local conditions, probably on account of a gradual yielding of adhesions.)

The last living case to which I will refer was that of a patient whose age was 42. She had first noticed the abdomen large

three and a half years before I saw her. It grew rapidly, and then all the signs of a rupture of a cyst occurred, namely, sudden violent pain followed in three days by complete disappearance of the swelling. In a few months the abdomen was again large, and again the tumour ruptured. This happened a third time. Two months before she consulted me the patient's abdomen was tapped, and three and a half gallons of clear fluid were removed. At the operation I found considerable omental adhesions, and the pedicle of the tumour was very broad. The second ovary was healthy.

This patient made a smooth convalescence. The temperature in the vagina rose to 101° F. the second evening, and was normal within a week. I have not seen her since she left my care a month after the operation, but she writes that she is much better than she ever expected to be. She says she cannot hold her arms up without a burning and aching in her side, and that she has "a great weakness." She cannot eat solid food. If she does she "is sure to bring it up again."

The patient, who was a widow, was, however, well enough to re-marry within two years of the operation.

Three Cases Dead.

Of the three patients now dead one has been referred to already in the earlier part of this paper. I thought the growth was malignant, but I explored the abdomen and removed a simple multilocular cyst. The patient was subject to frequent severe attacks of asthma, and was extremely emaciated and feeble. She had no asthmatic attack, however, whilst under my care, although convalescence was prolonged very much by the formation of a bedsore. Recovery was ultimately good. Two years and three months after the operation the patient died of bronchitis complicating an asthmatic attack, the illness being of only two days' duration.

One patient died of malignant disease six months after the removal of a semi-solid ovarian tumour. There were some adhesions to omentum, but no real difficulties were encountered in the operation or during convalescence. Both ovaries were removed. Within three months the patient suffered from severe and increasing constipation.

By the kindness of Dr. Corner, of Poplar, I saw the patient about a fortnight before she died, and she then had large masses of new growth in the upper part of the abdomen.

The third case in which death has occurred was also one of malignant disease. The patient was 35 years of age, and I had no doubt that she had an ovarian tumour. This had been diagnosed in India, but the patient had come home with the idea that she might be pregnant, and Dr. Champneys kindly saw her with me and confirmed the diagnosis of ovarian cystic disease. When I opened the abdomen I exposed an ordinary cystoma of the right ovary with a fairly small base. There were no adhesions. However, when I had tapped and extracted the cyst I found that there was a solid tumour attached to it by a narrow neck. This solid growth was the size of a very large orange and of a somewhat flattened oval shape. It was buried in the outer part of the broad ligament, and was pressed so tightly against the iliac vessels and the pelvic brim that I had considerable anxiety as to whether I should succeed in removing it and arresting hæmorrhage. I was able, however, to get away all the visible new growth more easily and completely than I expected, the broad ligament being secured in many pieces. On the left side there was a slightly smaller solid tumour buried in the broad ligament exactly like that on the right side. This was attached by a narrow neck to an apparently healthy ovary in precisely the same way as the solid tumour on the right side was attached to the base of the ovarian cyst on that side. That is to say, both the solid tumours were completely extra peritoneal between the folds of the broad ligaments. On the left side the ovary was attached to the broad ligament in the usual way, and the solid growth was attached to it between the folds of the broad ligament. On the right side the ovary was replaced by a cyst which was also attached to a solid growth by a narrow neck between the folds of the broad ligament. I also removed the left ovary and tumour. I have never seen any tumours which at all resembled these solid growths in their situation and connections. Microscopically they consisted of an epithelial growth of a papillary type.

I exhibited the tumours at the meeting of the Obstetrical Society on May 6th, 1896, and a note concerning them appeared in the Society's 'Transactions' for that year, p. 166.

The patient made a good recovery and returned to India in excellent health a few months later, but I warned the medical man who had sent her to me that her future must be looked upon with great anxiety. Eighteen months after the operation, the patient (having lost her husband very suddenly) was again in England, and she consulted me in May, 1897, when I found a return of growth. The tumour seemed to some extent movable, but it had very extensive attachments in the pelvis and to the uterus. I decided that it could not be removed. I expected to see the patient again in July; but on the 9th of that month I was informed that she had been placed by her brother, who is not a medical man, under the care of another surgeon, the physician who had sent her to me not having been consulted. The patient had been seen twice by her second surgeon with an interval of about a month. I was informed that he stated that the tumour was growing rapidly, that he asserted in the most positive manner that it was not malignant, and that it must be removed at once. Neither I nor the gentleman who sent the patient to me was consulted, and the facts stated in the 'Obstetrical Society's Transactions' must have been unknown to the surgeon under whose care the patient had been placed. He operated on July 7th, 1897, and I understand that the difficulties encountered were very much greater than he anticipated. The patient died the next day.

The operator was a man of very large experience. The incident proves the soundness of the opinion I put forward at the beginning of this paper, namely, that mistakes are to be expected in dealing with these cases, and that statistical papers are of no value unless this possibility is recognised and full details of all cases are given, whether an operation is performed or not.

The study of these and other cases has impressed upon me very strongly the importance of an early recognition and removal of ovarian neoplasms. When a tumour is buried deeply in the broad ligament at the beginning of its growth, it is only by the merest chance that its removal will become more easy as time passes. It is quite as likely that it will bury itself deeper, and in every case delay in operating exposes the patient to the onset of complications which may cause great difficulty and great danger when the abdomen is opened, or before an operation is arranged for.

I wish now chiefly to point out that when inflammatory conditions are set up around an ovarian growth, the risk of trouble *after complete recovery from the operation* is very much greater than it is when there has been no inflammatory disturbance. All the patients I have traced in the series under consideration in which the operation was simple, are practically in good health, with the exception of the one who died of asthma (and of course of the one that died of pneumonia since this paper was read); and those who are known to be in bad health now had all suffered from prolonged inflammatory attacks before being operated on. There can, I think, be no doubt that in a series of uncomplicated operations for simple ovarian tumours, the success ought to be very nearly, if not quite, complete and perfect; and therefore no opportunity should be allowed for the development of complications by delay in operating. If the possibility of the existence of a malignant element in the tumour be considered, the urgent necessity for early operation is still more obvious. Nevertheless, cases are even now seen in which, although ovarian tumours are recognised, either the patient is advised to wait till serious symptoms arise before being operated on, or the possibility of operative treatment is not suggested.

There are, of course, cases in which the patient on account of modesty or timidity, or simply because there has been no sign or symptom of mischief, does not consult a medical man until some complication forces her to do so. When, however, an ovarian tumour is diagnosed, it should be an absolute rule to have the question of its removal discussed at once. I have occasionally told a patient that I did not anticipate any harm from a delay of a few weeks, but the circumstances are very few in which it is advisable to postpone the removal of an ovarian tumour beyond the time necessary to make proper arrangements for the performance of the operation; and the reason for delay should never be that the patient is quite well. There must of course be always some risk in performing even the simplest ovariotomy, but this risk is smaller than that of allowing the tumour to remain. Although I am now chiefly urging an early resort to operative treatment in the interests of those patients who recover from ovariotomy, because the dangers of after trouble are so much less if the operation is uncomplicated, and although I had not intended to deal with any cases operated on beyond the period during

which the 50 referred to in the title of my paper were under my care, yet my unpublished fatal cases show so forcibly the increased immediate danger of an ovariotomy when the operation is too long postponed that I am only following out one line of argument by shortly recording the fatal cases of ovariotomy that have occurred in my practice, and which are not already published.

Fatal Cases not hitherto Published of Ovariotomy, and Operations Undertaken when Ovarian Tumours were Supposed to Exist.

In two of these cases the patients did not seek medical advice until forced to do so by their inability to go about their work any longer. The first was a case of a single woman, 53 years old, who had known for some years that she was growing very large in the abdomen, but she did not consult a doctor until 14 days before her operation, and she was admitted to the Samaritan Free Hospital as soon as this could be arranged for. I do not remember ever to have seen a patient look so ghastly as this one did. Her hair was dark; her face showed no blood anywhere; she could not breathe except sitting absolutely upright in bed, and she maintained this posture day and night by supporting herself on her hands. Her face, neck, and shoulders had little but skin over the bones, and her enormous belly increased by contrast the effects of her anæmia and emaciation. The temperature varied between 98·4° and 100° F. Her pulse was 130 to 140, and very feeble. She was obviously too ill to answer questions, and as she had lived alone no satisfactory history of the case was obtained. The abdomen was immensely distended, had a glazed shining appearance over the whole surface, and was very tender. It evidently contained a cystic tumour, which also distended the pelvis. Obviously the prognosis was very bad, but after a few days the abdomen seemed a little softer, and I thought it right to operate. I opened the abdomen on December 30th, 1898, and found a large tumour with recent adhesions to all the surrounding parts. Layers of white lymph, from a quarter to half an inch thick, covered its upper surface, and extended between the coils of small intestine, which were matted together in the upper part of the peritoneal cavity. I removed the tumour,

which was a suppurating multilocular ovarian cystoma, and I secured a thick pedicle on the right side in several pieces. Much lymph was taken away, and the abdomen was closed, a drainage tube being left in Douglas's pouch. In the evening, after the operation, the patient expressed herself as very comfortable. She could lie down and breathe comparatively freely. Her temperature was 100·8° F., and her pulse 136.

In the night the bowels began to act, and the patient collapsed and died rather suddenly between three and four o'clock next morning. After death it was found that the last 6 inches of the ileum were in a sloughing condition. It is quite possible, and I think likely, that a month before this operation was performed the patient had a very large ovarian tumour without inflammation, adhesion, or any complication, and that then an operation would have been almost free from danger. Tapping the cyst would have been as certainly fatal as the operation performed.

In the second case the patient, who was 50 years of age, had suffered much pain, and had known that her abdomen was very large for more than two years before she sought advice. I operated on March 17th, 1899. The right ovary consisted of a large suppurating tumour deeply embedded in the broad ligament and pressing the ribs firmly upwards. Its walls were so fragile that it was quite impossible to prevent the escape of its contents at the side of the tapping cannula. At the same time the walls were so thick and so little pliable that the cyst could not be drawn out partially until the adhesions behind were separated. I made a very long incision in the abdominal wall, but there were many strong adhesions high up in the peritoneal cavity, and fouling of the peritoneum by the pus from the tumour was unavoidable during the necessary manipulations.

When the bulk of the cyst was removed from the abdominal cavity, a deep enucleation from the broad ligament was necessary. The uterus was so completely atrophied and was drawn up so high on the tumour that it was mistaken for the end of the Fallopian tube, and was removed inadvertently. I noticed this at once, and the stump of the uterus was properly secured after tying the uterine arteries as in a case of hysterectomy. The left ovary consisted of a cystic tumour, with a good pedicle, quite free from the uterus. There were some slight adhesions, but this tumour was easily removed. I carefully cleansed the peritoneum,

and drained the pelvis by means of a glass tube. At first the patient seemed as if she would do well, but she died of septicæmia on the eighth day. Extensive purulent inflammation of the peritoneum was found after death, the intestines were much distended, and seemed, in places, as if they had been about to give way. The septic mischief must have developed from the pus spilt at the operation, or possibly a fresh dose of the poison escaped through the intestinal walls. The original cause of suppuration in the tumour had probably come from some adherent bowel. There can be no doubt that if an operation had been performed on this patient at an earlier date she would have had a very good chance of doing well.

In the third case the patient was 44 years of age, and her first trouble was a difficulty in passing urine two years before I operated. A catheter was required then and frequently afterwards, but the tumour did not seem to have been detected, and the patient did not notice any swelling. Two weeks before admission to the Samaritan Free Hospital the patient was living in Croydon, and was knocked down by a cab there. Dr. Wale of that town was called in, and at once discovered a tumour. It extended from the pelvis as high as the umbilicus, and dragged up the uterus so far that the tumour was much below the level of the os uteri. There was tenderness in the right groin after the accident, and the temperature was above normal, sometimes as high as 101·0° F. I operated on October 10th, 1899, and removed a multilocular cystic tumour of the left ovary, which filled the pelvis, and was firmly held there by suction and by adhesions. On the right side there was a very much swollen ovary. The fimbriated extremity of the Fallopian tube was occluded, and the tube itself was much distended. I removed the ovary and tube with great difficulty, without opening the tube. I could not find the vermiform appendix, but I suspected that it was buried in the adhesions from which I had removed the inflamed right tube and ovary. The parts removed seemed fairly clean, however, so I closed the abdomen, placing a drainage tube in Douglas's pouch. This was removed after 24 hours, as the amount of fluid escaping through it was very slight, and was quite sweet.

Mr. Cuthbert Lockyer kindly examined the right Fallopian tube for me, and got from it a pure culture of staphylococcus pyogenes aureus.

For the first 10 days this patient seemed to do well, except that she was remarkably sleepless. On the second and third days her temperature rose to 103·2° F. and 102·6° F. respectively, but on the tenth day after the operation it had been below 100° F. in the vagina for three days, and up to this time the condition of the abdomen seemed perfectly satisfactory, the bowels having been freely moved by enema on several occasions. The only difficulty was the persistent sleeplessness, for which it was necessary to give opiates. These secured the needed rest, but on the eleventh day the temperature again rose, the respiration became laboured, and there was slight expectoration. The weather was very cold and foggy, and I attributed the adverse symptoms to an inflammatory condition of the lungs. On the thirteenth day there was a suspicion of tenderness and fulness in the right groin, her temperature was 103·6°, and the patient died on the following day.

At the *post-mortem* examination it was found that the upper part of the abdomen was shut off from the lower by adhesions. Below, there was considerable peritonitis, with pockets of purulent lymph amongst the coils of intestine and along the ascending colon. The lungs were intensely congested and of a dark purple colour, the lower lobes being in a condition of nearly solid œdema. The heart was very soft and flabby, but the valves were normal, and there was no naked-eye evidence of fatty change.

The vermiform appendix was bound down by old adhesions, but was itself healthy. Probably, in this case there was an inflammation in the situation from which I removed the right tube and ovary, and this was at first shut off by adhesions; the sleeplessness was a result of absorption of poison; the enforced quiescence induced by opiates led to congestion of the lungs, and the peritonitis then made rapid progress. I think it is quite possible that until the lungs became congested, there was no pus in the abdomen. The fact that there was no distinct sign of mischief in the abdomen for 10 days is very unusual in my experience of ovariotomy, but an insidious onset of septic peritonitis in cases of inflammation of the vermiform appendix is not very unusual, and it is fairly certain either that I had not removed all the infected tissues or that fresh infection occurred later. During the operation, I thought of making a small opening in the loin and passing a gauze drain down to the position from which I had removed the right ovary. Probably this would have saved the

patient's life, but unfortunately I decided that I had removed all the infected parts. It is certain that if this patient had been operated on before she was knocked down by the cab, the condition would have been much more favourable; but up to that time the presence of a tumour seems to have been overlooked.

In the case of a patient, aged 60, I explored the abdomen on January 13th, 1897, and found that there were two malignant ovarian tumours with secondary deposits in the omentum. I closed the abdomen at once, and sewed up the wound without attempting to remove the tumours. Next afternoon, at 3.30 o'clock, every sign was favourable, and the patient said she was very comfortable. About half-past four, after a quiet sleep, she woke up in a state of extreme restlessness, with a quick pulse and much difficulty of breathing.

In my absence Mr. Doran and Mr. Corrie Keep kindly did all they could for the patient; but she did not rally, and she died somewhat suddenly at 8.20 o'clock.

At the *post-mortem* examination it was found that the tumours were attached to and invaded the walls of the intestine in several places, there being a mass the size of a bantam's egg in the cæcum. The omentum also contained much malignant deposit. Dr. Allchin made the *post-mortem* examination, and reported that the heart was "much dilated, soft, and flabby, and exhibited fatty invasion to an extreme degree, the left ventricular wall at places being almost replaced by fat."

There is little doubt that the cardiac condition was the cause of death. It is not possible to ascertain exactly the relations of an abdominal tumour in such a case as this without opening the peritoneal cavity. Probably no good could have been done in this case by operation at any time, unless perhaps at a very early stage of the disease, but the fatal issue would not have been likely to occur if the exploration had been done earlier. I have only had one other fatal case at all comparable to this one, and that also was a case of advanced malignant disease.*

In the following case I diagnosed an inflamed ovarian tumour. The patient was 30 years of age, had been married 11 years, and had borne six children. She had a soft indefinite swelling to the left of the pelvis, pushing the uterus over to the right and rising

* 'Lancet,' January 17th, 1899: "Some Cases of Malignant Disease associated with Ovarian Tumours," Case 7.

nearly to the level of the umbilicus. She had been delivered of a dead child seven months before I saw her, and she dated her illness from that confinement. During the 10 days the patient was in hospital before the operation, there was only a very slight rise of temperature, 99·6° F. being the highest recorded. I opened the abdomen on June 25th, 1897, and found the contents of the pelvis so matted together that I had great difficulty in making out the relations of the viscera. After separating much omentum and opening many cavities containing fluid, I succeeded in removing both ovaries and Fallopian tubes, which were very much inflamed. The pelvis was drained by a glass tube and by iodoform gauze. Although there was a high temperature early in this case, the abdominal condition progressed favourably, and the abdomen remained soft and flat. On the fourth day the left parotid gland swelled. It was incised on the fifth day, and discharged unhealthy pus until the patient died on the tenth day. After death numerous abscesses were found in the lungs, and the left parotid gland was in a sloughy condition. The peritoneum showed neither lymph nor pus formation except in the pelvis, the site of the operation being shut off from the rest of the peritoneal cavity by adhesions. So far as I am able to speak definitely, this was not a case of ovariotomy for new growth; but the operation was begun on the assumption that a tumour existed. The disease appeared to be an intense septic inflammation, beginning in the Fallopian tubes, and the infection was not completely removed.

Another fatal case is recorded here because I thought, before the *post-mortem* examination, that the mischief probably began in the ovary. The patient was a single woman, 34 years of age, who was sent to see me about the middle of October, 1899, by Mr. Coalbank, of Teddington. She had complained of increasing constipation for over a year; but she did not consult Mr. Coalbank until, by the advice of a friend, she had taken some liver pills, and so had made herself very much worse. I found a very hard growth in the right loin, about the level of the umbilicus, and an equally hard mass pressing low down in the pelvis on the right side. There was considerable intestinal distension, which made it impossible for me to estimate the size of these masses and whether they were connected or separate.

The intestinal distension increased, and I admitted the patient to hospital on October 31st. A distended coil of intestine was

then very obvious in the neighbourhood of the cæcum, and in spite of many lumps of fæces being removed from the lower bowel by enemata, this coil continued to increase in size rapidly until it was very tense and painful, and stood out prominently from the rest of the abdomen, which was also distended, but not to any great degree. I thought that the mischief depended on an ovarian new growth, and the existence of secondary deposits seemed probable. There was no prospect of my being able to do more than relieve the patient by the formation of an intestinal fistula, but surgical interference seemed necessary on account of the painful tension in the distended gut. On opening the abdomen in the right groin I found the omentum studded all over with new growth, and there were large masses in the pelvis. I opened the distended gut, which was the cæcum, and made a fistula. The obstruction was, however, only partially relieved; much fæces but very little flatus escaped; the general abdominal fulness did not go down, and the patient died early the next morning.

By *post-mortem* examination it was found that the malignant disease was much more extensive than I had thought. It appeared to have begun in the greater curvature of the stomach, where there was a considerable mass which had induced a malignant ulcer of the mucous membrane. The ovaries were normal, but large masses of secondary disease occupied the pelvis and were attached to the ovaries. The intestines and the peritoneum generally were affected at numerous points. It is remarkable that neither nausea nor vomiting was a prominent symptom in this case.

In one case there is room for doubt as to whether I removed an ovarian new growth or an abscess of the ovary.

I was asked to see the patient in July, 1897, by Dr. Walker, of Peckham Rye, and I diagnosed an inflamed left ovary and Fallopian tube. At this time it was not possible to say definitely that there was an enlargement of the ovary. The patient's general condition was good, and she had recently come home from India, where she had been a semi-invalid for many months. As complete rest had not been taken, except in a very intermittent way, I suggested that the patient should lie on her back for a couple of months, and be treated by counter-irritants and warm douches. The inflammation diminished, and all went well for six weeks.

The patient was then suddenly seized with acute symptoms. I was out of town, and when I returned a fortnight later I found that there was a large mass in the left side of the pelvis. It was acutely inflamed, and the patient had much fever of a hectic type. Without further delay, on September 29th, I removed the left ovary, which was of the size of a large cocoanut, and contained one large abscess cavity. The mass was very firmly adherent to several coils of intestine above it. For a few hours the patient seemed better, but she was already in a septic condition. Towards night she began to rave, and she died very early the following morning. If I had operated on this patient soon after I first saw her I should probably have performed an easy and safe operation. I think the case was one of acute infection of a chronically-inflamed ovary.

I must mention one other case to complete the list of my fatal cases of ovariotomy, and of those other fatal cases in which an ovarian tumour was or might have been diagnosed. I removed this patient's ovaries for chronic pain due, I believe, to the presence of a small fibroid tumour pressing on the left Fallopian tube. There was a small cyst of the right ovary which I had not diagnosed, but which makes it necessary for me to mention the case here. The patient died of hæmorrhage 12 hours after the operation. The case does not invalidate my argument that ovarian tumours should be operated on early. In cases of death from hæmorrhage after ovariotomy I should not like to say that the surgeon has always been to blame, but there can be no doubt that in most cases a somewhat different disposition of the ligatures, or a little more force used in tying them, would have saved the patient's life. I have been responsible for the treatment of secondary hæmorrhage twice in my own practice and once in that of a colleague during his absence, and I have had the good fortune to save two of these patients. Both are ignorant of the danger they passed through.

In a fourth case, not one of ovariotomy, I arrested secondary hæmorrhage and saved the patient's life for a time, but the details of the case have no bearing on this paper and will be published elsewhere.

There are a few causes of death after ovariotomy which, like hæmorrhage, may be described as accidental, and which are mostly preventable. If such causes of death be set aside, the

cases I have recorded in this paper clearly prove that delay in operating is a chief cause of fatal results in ovariotomy, and also of unsatisfactory conditions arising in patients who recover from this operation. I would urge, therefore, that it is the duty of medical men, whenever these tumours are diagnosed, to advise their removal without any avoidable delay.

In conditions such as the presence of a uterine fibroid tumour, in which there may be a reasonable prospect that the patient will not require an operation at all, there is often room for a difference of opinion as to whether an operation should be performed, and there are also cases of ovarian tumour in which an operation appears so formidable that it may not be considered wise to undertake it; but an ovarian tumour is sure to give trouble sooner or later, and it should therefore be removed as soon as diagnosed if such treatment seems at all practicable.

This is the only way in which patients can obtain the full benefit of the well-known advances which have been made in abdominal surgery.

January 8th, 1900.

EXCISION OF THE VESICULÆ SEMINALES FOR TUBERCULOUS DISEASE.

By C. Mansell Moullin, F.R.C.S.

In discussing the question of the operative treatment of the early stages of tuberculous disease of the bladder during the last session of the Medical Society, I suggested that, if in any case in which the disease was limited to one side it seemed probable that the infection was spreading upwards from the epididymis to the vesicula seminalis, it would be advisable to remove this organ as well as the testis, and in this way prevent, or at the least lessen the chances of, the extension of the tubercle to the prostate and the base of the bladder. I had before then removed one of the vesiculæ for chronic inflammation consequent upon gonorrhœa, and satisfied myself that the operation presented no exceptional difficulty, but I had not then met with a case of tuberculous disease in which such a proceeding as this appeared

to me to be suitable. Since then I have performed this operation twice with this object, and the result in each case has been excellent, the patient making a good recovery and, to all appearance, being freed from a source of infection which, if left, would have extended to the bladder and caused intense suffering and death.

One of these cases was at hospital. The patient was 22 years of age. Last Easter he noticed that the left testicle was becoming enlarged and painful. Three weeks before admission the skin of the scrotum had given way. On admission there was advanced tuberculous disease of the epididymis with a sinus in the scrotum. The vas deferens was thickened and the vesicula seminalis of the same side hard and nodular. There did not appear to be any tuberculous disease elsewhere. The testis was removed on July 4th. An oval incision was made in the scrotum so as to include all the involved tissues, and continued up the groin. The inguinal canal was laid open, the cord ligatured at its highest accessible point, the whole mass removed, and the wound sutured. The patient was then placed in the lithotomy position with the buttocks very much raised, and the under surface of the prostate exposed through the perineum by means of a modification of Zuckerkandl's incision. This consists of a transverse division of the perineum through or immediately behind the centrum tendineum, with a prolongation from each end backwards and slightly inwards into the ischio-rectal fossa, so far as may be found necessary. Instead of this I made a semilunar incision with the concavity towards the anus. This divides the superficial perineal vessels and nerves, but avoids any danger to the artery to the bulb unless this takes some unusual course. If more room is wanted the backward prolongation on the affected side can be made rather longer, but I have not found this necessary. The external sphincter is detached; the rectum pressed down into the concavity of the sacrum, and the fibres of the levator prostatæ divided. All that then remains is the layer of the recto-vesical fascia which passes behind the vesiculæ seminales, and keeps them against the posterior surface of the bladder. I have not found any difficulty with the prostatic plexus. If the division of this fascia is made in the middle line above, or on a level with the upper limit of the prostate, there is little or no fear of injuring it, at least, in young subjects. If now firm pressure is made over

the pubes by an assistant, the prostate, the posterior surface of the bladder, and the vesiculæ seminales are brought within easy reach of the finger, and can be manipulated without difficulty. It is better that the bladder should not contain any urine, and I have not found any benefit from a catheter tied in the urethra.

In the present instance the vesicula was separated from the under surface of the bladder, hooked well forward, and the vas deferens divided as high up as possible. In doing so the ejaculatory duct was torn out from the prostatic urethra, laying this open. The slit in the urethra was sutured with catgut; the wound plugged with iodoform gauze to prevent any venous oozing, and the sphincter ani united to the centrum tendineum by a silkworm gut suture. The superficial wound was closed with catgut. There was very slight shock. The temperature rose on one occasion to 101° F., but soon fell again. At first the urine came almost entirely through the wound. A fortnight after the operation he passed 25 ounces through the urethra, and after this it nearly all came the right way, but there is still a small fistula through which a few drops exude during micturition. The bowels were opened by enema five days after the operation, and there was never any loss of control over the sphincter. The part removed included the whole of the vesicula seminalis with the dilated portion of the vas deferens. The interior was full of breaking down caseous masses, but the disease did not appear to have penetrated through the wall, or to have involved the cellular tissue around.

The other case resembled this one so closely in all material particulars that there is no occasion to give a separate description. The epididymis was large and hard, but the caseous deposit had not yet broken down, so that there was no occasion to excise any portion of the scrotum. Two modifications were made in the method of operating. One was the removal of the whole of the vas deferens, part of which in the former case had been left behind. Two ligatures were used in the preliminary castration, one for the vas by itself, the other to include the other structures in the cord, and the vas was separated from its surroundings so far as it could be through the internal abdominal ring. The same was done for its lower end when the vesicula had been exposed through the perineal opening, and by careful traction the whole of the remaining portion of the vas was withdrawn together with

the vesicula. The other modification was the placing of a ligature around the ejaculatory duct to prevent any escape of urine into the wound. In the first case, I question whether this would have been feasible owing to the extent to which the vesicula was affected. In this one it succeeded in so far that the wound closed up without any trouble after the packing had been removed. The disease was of the same character, but hardly so far advanced as in the former case. Recovery was uneventful.

This operation does not appear to have been performed upon many occasions, and in the majority of these the immediate cause appears to have been induration and persistent pain consequent upon gonorrhœal inflammation. I can find very few cases recorded, none in England, in which it has been done for tuberculous disease, although there are a few in which caseous deposits have been scraped out. Ullman on one occasion removed both vesiculæ for tubercle; and in this case, as in the first one of mine, the ejaculatory duct was divided in the substance of the prostate, and a small fistula persisted. Roux has operated twice, and Fenger, Schede, and one or two others each once. Zuckerkandl's incision, or the modification of it which I adopted, appears to me to be by far the best. Roux recommends a lateral incision on the affected side, 2 or 3 centimetres from the middle line, reaching as far back as the ischium, and exposing the rectum freely. The patient is placed in the lithotomy position, but lying upon the side rather than upon the back. Pressure exerted from the rectum is made use of for the purpose of bringing the upper edge of the vesicula into the wound. But the small size and the great depth of the wound makes access very difficult. Von Dittel's incision, which follows the same direction, but begins with a curve in front of the anus, gives more space. But unless the perineum is very deep, or the suprapubic region very rigid, neither of which is probable in young subjects affected with tuberculous disease, there is no difficulty in exposing the part thoroughly through Zuckerkandl's; and this has the further advantage that if there is any question about the condition of the other vesicle, it can be examined without difficulty.

I do not suppose that this operation will be often performed. A fair number of cases of primary tuberculous disease of the vesiculæ seminales has been recorded, and these, of course, would be the most suited to an operation of this kind, but it is probable

they are not nearly so common as the number of those which have been published would imply. And the operation is not required when the disease is limited to the epididymis, and is almost useless when it has extended beyond the vesiculæ and invaded the surrounding structures. A portion of the prostate has, it is true, been excised at the same time in one instance, and in another a caseous focus was felt and scraped out; but this must be nearly the limit. There remain, however, a considerable number of cases in which, at a certain stage, the disease has extended into the vas, but has not yet involved the prostate to any serious extent, or the base of the bladder; and these cases can be diagnosed with a very fair degree of certainty, for the existence of tuberculous deposit in the vesiculæ can be felt without any difficulty through the rectum; in such as these I believe this operation to be not only justifiable, but strongly to be recommended and even to be urged, provided, of course, the lungs and other organs are not seriously involved. Without exposing the patient to too great a risk it gives him one more chance of being freed from a disease which, when it has once reached this stage, will, if it is left to itself, inevitably progress and ultimately cause death with the greatest suffering.

Mr. Swinford Edwards remarked on the novelty of the subject, at any rate in this country. He said that he had seen one case in which the operation had been done for tuberculosis of the vesiculæ seminales, and he thought the operation was by no means a simple one, as it took a good deal of time and the hæmorrhage was considerable. It occurred to him that the vesicle might be exposed by a modification of the operation devised by Kraske for excision of the rectum. He asked the author whether he would be disposed to recommend the procedure for cases of gonorrhœal vesiculitis. He himself had treated one such case by massage per rectum. He also asked whether it was not likely that a fistula might result from such an operation.

Mr. F. C. Wallis thought the author had not sufficiently insisted on the difficulties inherent to such an operation. These difficulties, it was true, might not present themselves to an operator of the author's experience, but they were nevertheless considerable.

Mr. Mansell Moullin, in reply, said he had cut down upon the prostate four times in cases of enlargement, &c., and if the patient were put in the lithotomy position the parts showed themselves perfectly through a transverse incision into the perineum. In old men there was a certain difficulty on account of the plexus, but in younger subjects, provided one kept to the middle line and pushed the plexus on either side, there was very little hæmorrhage, as only the muscles and a single layer of the recto-vesical fascia had to be divided. As soon as that was done, the posterior surface of the prostate, the vesiculæ seminales, and the base of the bladder came out quite plainly. It was easier to perform

the operation upon a young than upon an old man ; for in the latter class of cases pressure from above the pubes produced very little effect, whereas in the young, and especially those who were the subjects of tuberculous disease, there would be no superabundance of fat, and the assistant operator could drive the base of the bladder and the vesiculæ seminales right down to the perineum. He explained, in regard to the case in which he had excised the prostate for gonorrhœal vesiculitis, that the patient had been under treatment for 18 months, and had been to America, where he had been subjected to the so-called "stripping" of the vesiculæ seminales. A certain amount of relief followed, but the patient was not cured, and still suffered severe pain. In this case the operation was more difficult than in either of the tuberculous cases, because the inflammation had spread through the walls of the vesicle, and had tied it down so closely to the base of the bladder that he had to remove it piecemeal. The patient, however, recovered without any trouble, and was relieved of the pain. He had seen him 12 months after the operation, and so far, at any rate, there had been no recurrence. In his experience Kraske's operation was attended with a much greater amount of shock than this one, and it occupied much longer time. The under surface of the bladder could be exposed sufficiently by means of a semilunar incision across the perineum without carrying the incision further back, and, above all, without interfering with the sacrum.

SOME REMARKS ON THE OPERATIVE TREATMENT OF UTERINE FIBROIDS.

By W. J. Gow, M.D.

Fibroid tumours of the uterus are, unfortunately, of very frequent occurrence, and whilst, on the one hand, they may sometimes lead to but little trouble, they may, on the other, seriously threaten life. Most often, however, they are a source of serious discomfort and ill-health, without actually endangering life; and though in a considerable number of cases the symptoms which they give rise to can be relieved to a very remarkable extent by medicinal treatment, yet it not infrequently happens that the relief is only partial, and sometimes no real improvement follows even the prolonged use of drugs. The question of operative treatment, therefore, becomes an important one, and there can be no doubt that, of all the operations devised for dealing with this condition, hysterectomy is the one that is most certain to give complete and permanent relief. The chief factor which has kept this operation under, has been the high mortality which has attended upon it, and even now by many it is regarded as one involving great risk. The sphere of usefulness of this operation

would be greatly increased if it could be shown that the mortality from it need not necessarily any longer be a high one. The mortality of the operation for the removal of a uterus which is the seat of fibroid tumours is a matter, to my mind, of the utmost importance, because on the mortality of the operation turns the whole question of the indications for hysterectomy. So long as the mortality of the operation remained as high as 20 per cent., it is obvious that it was impossible to advocate removal unless as a last resort, and the patient was well advised to put up with the troubles she had rather than run the very considerable risk of losing her life which the operation exposed her to. It has often been insisted upon that fibroids seldom threaten life, but it is equally true that they often reduce the possessor of them to a condition of chronic invalidism, although after years of waiting the menopause may arrive and bring with it some measure of relief. If hysterectomy had no mortality, or if the mortality was very low, it is clear that the operation might be honestly recommended in cases where, although the life of the patient was not threatened, the health was so seriously affected as to lead to inability to perform the ordinary duties of life with pleasure or efficiency. To a poor woman who is in part or entirely dependent on her own exertions, long-continued ill-health resulting from excessive monthly losses of blood is a serious matter, and the consequent inability to gain her livelihood renders waiting for the menopause an irksome or impossible task. Even among the well-to-do classes the loss of the best years of their life is hardly to be contemplated with equanimity, and if by a comparatively safe operation complete and permanent cure can be obtained, it is hardly fair to restrain them from seeking it. It is quite true that if the immediate risk to life from an operation is great, it is better to wait rather than to run such a risk; so that the whole question of operative treatment turns on what that risk is.

The operation of abdominal hysterectomy will always be a formidable one, and often a difficult one, and one never to be undertaken lightly. A certain amount of shock is hardly to be avoided, and the opening up of connective tissue spaces will make blood poisoning easy, unless sufficient precautions are taken. If, however, the seriousness of the operation is fully realised, and every precaution, which modern science has shown to be essential, is taken, the risks of this operation may be reduced almost to

a vanishing point. The operation is often not simple and not easy, and if performed by those inexperienced in abdominal surgery, or in unsuitable surroundings, or without every antiseptic precaution, it will still undoubtedly be attended by a disastrously high death rate. In such an operation as hysterectomy the penalty of inattention to detail is heavier than in the simpler one of ovariotomy. It is not uncommon for operators to still talk of a mortality of 10 per cent. as if it was a necessary evil which could not be avoided, but such a death rate is a very terrible thing, and it is only when we realise the fact that there is no legitimate mortality in hysterectomy, or any other operation, that it will be reduced to within reasonable limits. An occasional accident will, I suppose, be always liable to occur, but apart from such a misfortune there should be, and I believe in time there will be, no mortality at all as the direct result of surgical operations.

If fatal cases are carefully studied, and they deserve study as much or more than those which are successful, the operator will generally be able to find out the reason in some fault of omission or commission.

As to the relative advantages of the extra- and sub-peritoneal treatment of the stump the whole question again turns on the relative mortality of the two operations. Life is a more important consideration than any other, and if more patients live after the use of the serre-nœud than after dropping back the stump into the abdomen, no reasonable person would adopt any other method. It is for the advocates of the sub-peritoneal plan to show that their mortality is as low as that of the advocates of the older extra-peritoneal method, and it is on the question of mortality, and on nothing else, that either operation can claim to rise or fall. It is not necessary to show that the mortality of the sub-peritoneal method is lower than that obtained by the use of the serre-nœud, because the former possesses certain intrinsic advantages of its own, such as a more comfortable convalescence, and a firmer abdominal scar. Personally, I am a strong advocate of the sub-peritoneal plan, and in my own experience the mortality from this operation has been much less than that of the extra-peritoneal, and it has a further great advantage in that it is applicable to all cases. This is a very important point, because I do not think it can be said of any of the other radical operations for the removal of a fibroid uterus, such as pan-hysterectomy

Since September, 1895, I have employed this method in all cases of abdominal hysterectomy for fibroids, and during that time I have never had to fall back on oophorectomy as a substitute. That is to say, that in all cases where I set out with the object of performing an abdominal hysterectomy, I have always been able to do it by this method. I cannot help thinking that those surgeons who habitually use the serre-nœud meet from time to time with cases where, owing to the absence of any kind of pedicle, they content themselves with removing the ovaries instead of removing the uterus. In my series of 47 cases, which form the basis of my remarks this evening, there has been no selection. They represent the entire number of patients (both hospital and private) on whom I have operated for uterine fibroids during the last four and a half years, with the exception of three or four cases where vaginal enucleation was practised for the removal of purely submucous tumours. During that period I have never done an oophorectomy in a case of fibroids. In all the cases the uterus with the tumour, or tumours, was removed at the level of the os internum, and the stump, after being dealt with in the ordinary way, was dropped back into the abdomen. Drainage was only employed on one occasion (Case 40), the details of which I shall narrate shortly.

Among those 47 cases there has been one death (Case 8), and the table shows a series of 39 consecutive cases of recovery, giving a total mortality of just over 2 per cent.

My excuse for bringing forward this series of cases is that I hope to prove that abdominal hysterectomy properly performed is not so dangerous an operation as it is generally thought to be, and further that the sub-peritoneal treatment of the stump gives as good or, I believe, better results than any other plan. At a meeting of the Obstetrical Society of London, held in November, 1897, Mr. Bland-Sutton read a paper, entitled "On Abdominal Hysterectomy for Myoma of the Uterus: with Brief Notes of 28 Cases." In all these cases the sub-peritoneal plan of treating the stump was adopted. Two of the cases died, but he was able to show a series of 20 consecutive recoveries. Mr. Meredith, in the discussion which followed, stated that the serre-nœud had given him 83 recoveries out of a succession of 90 abdominal hysterectomies (a mortality of 7·7 per cent.), including a run of 30 cases without a fatality. He claimed at that time that these

results had not as yet, as far as he knew, been equalled by the advocates of the sub-peritoneal method. As I have shown in my own series, these results, admirable as they are, can not only be equalled, but can be surpassed by the sub-peritoneal method, and no doubt Mr. Sutton and others could now produce series of cases considerably longer than my own in support of the same view, though, as far as I know, no such tables have been published in this country up to the present time. It is often said that the lessened mortality of to-day in such operations is due to the fact that it is done early, when the tumour does not reach higher than the navel. Although I certainly advocate the removal of tumours of this size under certain circumstances, I am sure that the operation is no easier than when the tumour is large and fills the abdomen, and this factor cannot be called into account for the small mortality. In fact, speaking broadly, the larger the tumour the easier it is to remove, and an easy operation means a shorter operation, and, therefore, a lessened amount of shock. The success of this operation depends on two things, and two things only, viz., strict cleanliness and complete control of bleeding vessels.

Indications for the Operation.

Even though the mortality of abdominal hysterectomy is reduced to 2 per cent. or less, it is obvious that we cannot recommend such a form of treatment in all cases of uterine fibroids. At a rough estimate probably not more than 10 per cent. of all cases of fibroids call for an operation such as this. Presuming for a moment that the mortality could be kept as low as this, even then it is not easy to lay down very definite rules as to the indications for operation. Large size, rapid growth, free hæmorrhage, great pain, or serious pressure symptoms are obvious indications. Pain is not a very common symptom of fibroids, but if present is, in my experience, generally incapable of relief by drugs. In its most severe form it is generally due to one or other of the fibroids projecting into the uterine cavity and exciting irregular and painful contractions of a labour-like character.

The condition of life and the character of the patient must be taken into consideration. As has already been mentioned, poor women cannot afford to be partial invalids, and to some of those in easy circumstances ill-health is very irksome. To these opera-

tion may be recommended, though the symptoms are not severe enough to justify one in urging it as a necessity. To some women, on the other hand, an operation presents so many terrors that they prefer to remain as they are, so long as their life is not actually threatened. In many cases all one can do is to point out to the patient that her choice lies between putting up with the symptoms she has got or submitting to an operation which is obviously not devoid of risk, but which if successful will result in a complete cure.

Tumours that fill the pelvis generally require removal sooner or later, and if they are already causing discomfort there is no object in waiting.

A question that it is often useful to ask one's self is, whether it is probable that the patient will be able to go through the rest of her menstrual life if nothing is done. If the answer is in the negative, then there seems to me to be really no object in postponing what later will become an operation of necessity. For instance, a woman of 35 years of age has a fibroid which reaches to the umbilicus, and is already the cause of some pain, and a free monthly hæmorrhage and medicinal treatment give but little relief. It is extremely improbable that she will be able to pass through the next 15 years of her life with even a reasonable amount of comfort, although the urgent need for an operation may not arise until she is 42 or 43. With great care she may be able to hold out till then, but during the whole of that time she will have to live the life of an invalid. As the mortality now stands, I think it is better to advise an immediate operation, and thus avoid the loss of many years of activity. It is true, as already pointed out, that it is no safer and no easier to remove a fibroid uterus at this stage than it will be when it is twice the size, and even the anæmia produced by the loss of blood does not, unless very extreme, appreciably increase the risk of operation. But it puts the patient back into the world again at five and thirty instead of at five and forty, when invalid habits may very probably have become confirmed. To sum up, the lessened mortality of abdominal hysterectomy enables one not merely to save patients from death but to save them from invalidism. Any patient who is seriously incommoded by a fibroid which does not yield to treatment and is not suitable for enucleation through the vagina may fairly be advised to submit to an abdominal

hysterectomy with sub-peritoneal treatment of the stump, provided the would-be operator has reduced his mortality to below 5 per cent. My own method of operating in these cases differs slightly from that employed by many others, and I therefore venture to describe the plan as now adopted by me, which I find applicable to all cases.

Various minor details have obtained a fictitious importance. Baer, for example, insisted that no ligature should be passed through uterine tissue, and attributes some of his success to this rule. Others have insisted on the importance of excising or sterilising the cervical mucous membrane. In the last 39 cases I have invariably ligatured uterine tissue although the uterine arteries have been controlled by a separate ligature, and I have hardly ever excised or treated the cervical mucous membrane.

The plan of procedure has not been uniform, because with increasing experience certain modifications have naturally crept in. In the last 30 or 40 cases the following plan has been adopted :—After the peritoneal cavity is opened the tumour, when possible, is drawn out through the abdominal incision. If the broad ligaments are very short or the tumour is mainly intra-ligamentous and intra-pelvic, it sometimes happens that this cannot be done at this stage. Under any circumstances the first step in the operation is to ligature each round ligament about on a level with the os internum. The anterior flap of peritoneum is then marked out with a knife, and the round ligaments are divided on the uterine side of the ligature. This opens up the broad ligament, and with scissors the anterior layer may be divided, carrying the incision up towards the middle of the Fallopian tubes. The incision across the front of the tumour must be made sufficiently high to allow of the formation of a large anterior flap, and this flap is now stripped down with the fingers or the handle of a knife. The ovarian arteries and veins are next tied on each side by a ligature which includes the Fallopian tube. This ligature, at any rate on one side, should be tied so that at least one ovary is left, but if the fibroid occupies one broad ligament it is often more convenient to tie it on this side outside the corresponding ovary. Both ovaries should never be removed unless they are the seat of serious disease. Having tied the ovarian vessels on each side, the broad ligament is divided on the uterine side of the ligature. Clamp forceps may be used to check the oozing which comes from

the uterine end of the divided tissue. This is done on both sides, and then the posterior flap is marked out with a knife and dissected downwards for a short distance. The uterine arteries can now be generally felt without difficulty and may be seized with a pair of pressure forceps at the level of the os internum. The tumour is now cut away with a knife at the appropriate level and the stump caught up and drawn to the surface with volsellum forceps. The uterine arteries are now carefully tied. This plan takes less time than if the arteries are tied before the tumour is cut away.

After this has been done I always introduce what I call a precautionary ligature. A curved needle on a handle armed with silk is thrust through the stump from before backwards, avoiding the peritoneum so as to allow the ligature to include in its grasp the outer fourth of the stump. This is tied on the outer side, and includes the uterine artery, which, however, has already been tied separately. A similar ligature is now tied on the other side. The object of these ligatures is to control oozing from the cut surface. Vessels are given off obliquely, not horizontally from the uterine artery, and therefore, although this artery has been tied on the level of the cut surface of the stump, there may be some bleeding point owing to a branch springing off from the main artery below the point where it is secured, and running upwards and inwards. Such vessels are often seen, especially on the posterior half of the stump. By this means all oozing or spurting, as the case may be, is generally stopped at once, and even if there is no oozing it is a safe precaution, as no harm results from thus enclosing uterine tissue in a ligature. Vessels in the substance of the cervix should never be tied on the face of the stump, as the ligatures will almost certainly come off. If oozing continues, as it will sometimes in a very vascular stump, a curved Hagedorn's needle may be used to encircle the bleeding area, and the tissue and vessels compressed with a ligature. I think these additional precautionary ligatures are of value, because in the one fatal case which I have had (Case 8) death resulted from secondary hæmorrhage 36 hours after the operation as the result of the slipping of a ligature applied to a small vessel on the posterior part of the stump. In this case both uterine arteries had been tied in the ordinary way, but no precautionary ligatures had been used. At the *post-mortem* examination the uterine and

ovarian arteries were securely tied, but about a pint of blood had escaped from a vessel on the posterior part of the stump, which, at the time of the operation, had been secured by a superficial ligature. Since then I have always employed these lateral ligatures, and have never found that they have caused the least harm, and they afford a rapid and effectual means of checking oozing. In no case have I ever seen any ligatures discharged either through the abdomen or through the vagina.

In the fatal case above mentioned the patient did very well for the first 36 hours, but in the middle of the second night she suddenly became faint and showed symptoms pointing to internal bleeding, and sank in a few hours before I had the chance of seeing her.

I may add that probably the best way of dealing with secondary hæmorrhage from the stump, if it does occur, would be to excise the stump per vaginam, using clamps to control the uterine arteries. This would entail considerably less shock than reopening the abdomen, and could be done rapidly.

After the oozing has been stopped I generally put in two fore-and-aft sutures through the muscular substance of the stump, again avoiding peritoneum, and in this way the raw surface is rolled together. They probably are of no great importance, but they serve to hold the stump up with whilst the peritoneal flaps are united. After this has been done the peritoneum is cleansed and the abdomen closed.

May I offer a plea in favour of the simplification of abdominal operations? Personally, I seldom use more than six pairs of pressure forceps, and never more than six sponges. Further, I never have more than one assistant, and I never allow anyone but myself to touch my instruments and ligatures. I will not trouble you with other details as regards the management of sponges, but I hold that as few people should come within the circle of the operation as possible.

The desirability of leaving the ovaries seems still to be doubted by some, and we are told that, after the removal of the uterus, they undergo slow atrophy, and thus a real menopause develops in spite of their presence. Even if this is the case it is still, in my opinion, highly desirable to leave one or both of them, because, by so doing, the acute development of the climacteric is avoided. During their gradual atrophy the climacteric disturbances are in

no way greater than when they supervene as the result of a gradual loss of physiological activity. Although I have not been able to follow all my cases since operation, yet I have seen many of them, and in no case have I met with any serious mental or bodily disturbance such as so often follows after oophorectomy.

The most serious complication at the time of operation is the presence of old adhesions between the enlarged uterus and the adjacent structures, such as may result from a previous attack of perimetritis secondary to tubal disease.

In four of my patients (Cases 36, 39, 40, 42) a single or double pyosalpinx existed, and, inasmuch as this condition is somewhat rare, I propose to narrate briefly the chief features of these cases :—

Case 1.—This case was first seen by me on January 9th, 1899, in consultation with Dr. Sunderland. The patient was a widow, and had never been pregnant. During the previous October and November she had suffered from abdominal pain accompanied by some pyrexia, and had been obliged to remain in bed for several weeks. On December 24th she was suddenly taken ill with severe abdominal pain and vomiting, and the temperature rose to 104° F. Since then the pain had become less severe, but the temperature still remained between 100° and 101° F. The patient is markedly anæmic, and the monthly periods are profuse. On examination, a fibroid tumour was found reaching up to the umbilicus, but nothing further could be made out.

The operation was performed on January 12th, 1899, in a nursing home. Both tubes were found to be enlarged and adherent, and contained pus. Both ovaries were enlarged to the size of hen's eggs, and contained pus. The enlarged ovaries lay deep down by the sides of the supra-vaginal cervix. The uterus was first cut away to give more room, and then the appendages were dug out after much difficulty. Pus escaped from both tubes and ovaries during removal. No drainage was employed. Convalescence was normal.

Case 2.—This patient, who was 50 years of age, was admitted to the Royal Hospital for Children and Women in June, 1899, complaining of profuse irregular hæmorrhage. A large tumour, the size of a six months' pregnancy, occupied the abdomen. On vaginal examination a tumour the size of an orange could be made out in Douglas's pouch, and another similar tumour lay in front of the cervix. At the time of the operation the abdominal tumour was found to be the enlarged uterus, and the other two tumours were the Fallopian tubes distended with pus. The uterus, with both ovaries and tubes, was removed. The convalescence was remarkably smooth.

Case 3.—This patient was 35 years of age, and was seen by me with Dr. Squire at the Paddington Infirmary. She had been taken ill three weeks before admission with severe headache and abdominal pain, and a week later she became delirious, and remained so till after the operation. When seen by me the abdomen was distended and tender, the temperature was raised, the tongue was dry and brown, and there was noisy delirium.

A fibroid tumour reached as high as the navel. The cause of her illness was obscure, but as her condition was very grave, an operation was decided on. I performed hysterectomy on July 3rd, and both appendages were then found to be enlarged and very adherent. They were removed with great difficulty, and found to contain offensive pus. There was free oozing from the pelvis, and a drainage tube was therefore inserted. The oozing continued for the first 24 hours after the operation. Two days later the discharge from the tube became offensive, and remained so for several days, but ultimately the patient got quite well.

Case 4.—The patient was 42 years of age, and was admitted into St. Mary's Hospital for severe menorrhagia and abdominal pain. There had been pyrexia for some weeks before admission, and the patient was very ill. There was a fibroid tumour which reached up to the navel, and in the left posterior quarter of the pelvis a smooth, elastic swelling could be felt, the size of a hen's egg. At the operation, September 14th, 1899, the left tube was found converted into a pyosalpinx ; it, together with the uterus, was removed. Both ovaries were left. There was a good deal of oozing from torn adhesions, but no drainage was employed. The patient made a good recovery.

It has been impossible, in the time at my disposal, to do more than touch on some of the points of interest in connection with this subject; but my chief object has been to support the view that of all the radical operations for fibroids, abdominal hysterectomy, with sub-peritoneal treatment of the stump, is the best and the safest, and that this operation may confidently in the future be looked to to give a mortality of not more than 1 or 2 per cent.

Table of Forty-Seven Cases of Abdominal Hysterectomy for Fibroids with Intra-Peritoneal Treatment of the Stump.

No.	Age.	Date of Operation.	Place of Operation.	Result.	Remarks.
1	39 (M.)	Sept. 24, 1895 ..	St. Mary's Hospital ..	R.	Tumour (a soft myoma) reached to ensiform cartilage; much pain and hæmorrhage; patient very ill, great emaciation and nocturnal delirium; ovaries removed with tumour. Convalescence normal.
2	41 (S.)	Oct. 29, 1895 ..	Royal Hospital for Women and Children	R.	Pain, wasting; rapid growth of tumour; tumour reached to navel; tumour and ovaries removed.
3	25 (M.)	Feb. 18, 1896 ..	Do. do.	R.	Pain; rapid growth of tumour; tumour reached just above navel; tumour and ovaries removed.
4	40 (S.)	April 15, 1896 ..	Metropolitan Hospital..	R.	Menorrhagia and pain; tumour removed; both ovaries left.
5	48 (S.)	April 28, 1896 ..	Royal Hospital for Women and Children	R.	Menorrhagia and pain; retention of urine; both ovaries left.
6	27 (M.)	May 5, 1896 ..	Do. do.	R.	Menorrhagia and pain; tumour filled the pelvis and rose as high as the navel; tumour mainly intra-ligamentous; one ovary removed.
7	33 (M.)	June 30, 1896 ..	Do. do.	R.	Metrorrhagia; intense pain; some pyrexia; portion of one of the fibroids, which was sub-mucous, was found to be necrosed; one ovary removed.

No.	Age.	Date of Operation.	Place of Operation.	Result.	Remarks.
8	48 (W.)	August 8, 1896..	Metropolitan Hospital..	D.	Menorrhagia and pain; tumour filled the pelvis and rose nearly to the navel; cause of death was secondary hæmorrhage 36 hours after the operation.
9	44 (W.)	August 14, 1896	Royal Hospital for Women and Children.	R.	Menorrhagia and pain; both ovaries were found to be cystic and were removed with the uterus.
10	42 (M.)	August 25, 1896	Do. do.	R.	Menorrhagia and pain; tumour partly intra-pelvic; one ovary removed with the uterus.
11	38 (M.)	Sept. 1, 1896 ..	St. Mary's Hospital ..	R.	Large soft myoma; patient was three months gravid; both ovaries left.
12	35 (M.)	Sept. 8, 1896 ..	Royal Hospital for Women and Children.	R.	Rapidly growing intra-ligamentous tumour occupying pelvis and reaching nearly to navel. Both ovaries left.
13	29 (S.)	Sept. 16, 1896 ..	Do. do.	R.	Rapidly growing large soft myoma; ovaries left.
14	37 (M.)	Sept. 19, 1896 ..	Do. do.	R.	Rapid growth; menorrhagia; severe pain; tumour intra-ligamentous; ovaries left.
15	36 (M.)	Sept. 28, 1896 ..	St. Mary's Hospital ..	R.	Multinodular fibroids reaching to navel; had recently miscarried; ovaries left.
16	35 (M.)	Oct. 8, 1896 ..	Royal Hospital for Women and Children.	R.	Severe pain; retention of urine; part of tumour occupies pelvis; reaches to navel; double hydro-salpinx; tumour and both tubes removed; ovaries left.

17	36 (M.)	Dec. 8, 1896 ..	Do. do.	R.	Eight months gravid; many fibroids, one of which blocked the pelvis; child removed by cæsarean section and uterus amputated; child alive and well; one ovary removed.
18	40 (M.)	March 2, 1897 ..	St. Mary's Hospital ..	R.	Pain; bladder trouble; menorrhagia; tumour reaches above navel; ovaries left.
19	47 (M.)	Sept. 1, 1897 ..	Metropolitan Hospital..	R.	Menorrhagia and pain; ovaries left.
20	44 (S.)	Sept. 2, 1897 ..	Royal Hospital for Women and Children.	R.	Menorrhagia and pain; tumour reaches to navel, and extends into pelvis; one ovary removed.
21	38	Oct. 1, 1897 ..	Private house	R.	Profuse menorrhagia; very anæmic; tumour reaches to navel; adhesions to cæcum; one ovary removed.
22	47 (S.)	Nov. 23, 1897 ..	Royal Hospital for Women and Children.	R.	Menorrhagia and pain; tumour reaches above navel; intra-ligamentous; one ovary removed.
23	36 (W.)	Jan. 25, 1898 ..	Do. do.	R.	Menorrhagia; tumour reaches half-way between pubes and navel; soft myoma; ovaries left.
24	35 (M.)	Feb. 26, 1898 ..	Do. do.	R.	Rapid growing tumour, reaching to navel; intra-ligamentous cystic myoma; double uterus; one ovary removed.
25	45 (W.)	March 29, 1898	Do. do.	R.	Profuse hæmorrhage; very anæmic; intra-ligamentous; one ovary removed.
26	32 (S.)	April 6, 1898 ..	Do. do.	R.	Menorrhagia; rapid growth; tumour intra-ligamentous; one ovary removed.
27	46 (S.)	May 17, 1898 ..	Do. do.	R.	Rapid growth; one ovary removed.

No.	Age.	Date of Operation.	Place of Operation.	Result.	Remarks.
28	43 (W.)	June 4, 1898 ..	Royal Hospital for Women and Children.	R.	Profuse menorrhagia; both ovaries left.
29	40 (M.)	July 13, 1898 ..	Do. do.	R.	Increasing menorrhagia; very anæmic; one ovary removed.
30	45 (S.)	Oct. 4, 1898 ..	Do. do.	R.	Pain and menorrhagia; tumour reaches to navel; intra-ligamentous; one ovary removed.
31	41 (M.)	Nov. 1, 1898 ..	Do. do.	R.	Profuse menorrhagia; very anæmic; tumour to navel; both ovaries left.
32	48 (W.)	Nov. 23, 1898 ..	Metropolitan Hospital..	R.	Profuse menorrhagia and metrorrhagia; tumour to navel.
33	32 (S.)	Nov. 25, 1898 ..	Home	R.	Menorrhagia; abdominal pain; tumour to navel; ovaries left.
34	49 (M.)	Jan. 3, 1899 ..	Royal Hospital for Women and Children.	R.	Menorrhagia and metrorrhagia; tumour midway between pubes and navel; ovaries left.
35	51 (M.)	Jan. 6, 1899 ..	Do. do.	R.	Severe pain; irregular bleeding; large tumour showing patches of calcification.
36	44 (W.)	Jan. 12, 1899 ..	Home	R.	Menorrhagia; pain; pyrexia; tumour to navel; suppuration of both tubes and ovaries; appendages removed with tumour.
37	38 (S.)	April 12, 1899 ..	Metropolitan Hospital..	R.	Multinodular fibroids; profuse menorrhagia; great anæmia.

38	48 (S.)	April 24, 1899 ..	Royal Hospital for Women and Children.	R.	Tumour very large; œdema of legs; dyspnœa; cystic fibroid.
39	50 (M.)	June 16, 1899 ..	Do. do.	R.	Metrorrhagia; tumour to navel; both tubes greatly distended with pus.
40	35 (M.)	July 3, 1899 ..	Paddington Infirmary..	R.	Great abdominal pain and tenderness; patient delirious, febrile typhoid state; both tubes contained pus; much oozing; drainage employed.
41	42 (M.)	August 31, 1899	St. Mary's Hospital ..	R.	Pain and menorrhagia; tumour partly intra-pelvic.
42	42 (M.)	Sept. 14, 1899 ..	Do. do.	R.	Pain; menorrhagia; attacks of local peritonitis; left tube contained 3 ounces of pus.
43	45 (M.)	Sept. 26, 1899 ..	Royal Hospital for Women and Children.	R.	Menorrhagia.
44	44 (M.)	Oct. 11, 1899 .	Do. do.	R.	Rapidly growing tumour.
45	35 (S.)	Oct. 31, 1899 ..	Do. do.	R.	Very large tumour, size of full-term pregnant uterus.
46	40 (S.)	Nov. 10, 1899 ..	Home	R.	Tumour to navel; pain; rapid growth.
47	38 (M.)	Dec. 12, 1899 ..	Royal Hospital for Women and Children.	R.	Rapid growth; much pain; four weeks' ovum in uterus; developed phlebitis of left leg on tenth day after operation.

Dr. Blacker said that the author's brilliant results had, he believed, never been surpassed in this country and in but few instances in America or on the Continent. Howard Kelly's statistics showed a mortality of two in 100 consecutive abdominal hysterectomies. No doubt at the present moment the question as to what method of treatment to adopt in these cases was in an entirely different position to that of even a few years ago. They had been brought up in the idea that fibroids were not dangerous to life, and might safely be left to nature. Now, however, that the mortality of hysterectomy was as low as that after ovariotomy, no doubt there were many patients who ought to be operated upon. He was interested in the author's observation as to adhesions being the worst complications they had to fear, and he recalled a case of his own, that of a woman with a large fibroid tumour, who on admission was almost moribund from septic poisoning. On opening the abdomen the tumour was found to be universally adherent to the intestines and to the abdominal wall, and the patient's condition was so grave that its removal could not be thought of. She survived about 12 days, and, *post mortem*, there was found a large universally adherent fibroid tumour containing about three-quarters of a pint of pus in its interior. Personally, he followed Kelly's method of operating, but, to all intents and purposes, the author's method was that adopted by most operators in this country. He specially commended the precaution of placing additional ligatures through the cervical tissue, as it no doubt prevented a good deal of oozing. He thought the operation was one that could be confidently recommended to patients at the present day, as with proper antisepsis the risk was really a very small one.

Mr. Wallis said he had been greatly struck by the extreme simplicity of the author's methods, wherein no doubt lay the secret of his success. When one saw with what a small number of instruments he was content, one could not but feel surprised at the wonderful number of instruments and assistants which some operators thought necessary. He was sure that the success of these operations depended on their simplicity and upon the sterility of the instruments and of the hands of the operator.

Dr. Gow, in reply, said that the chief difficulty which adhesions gave rise to was bleeding during the course of the operation. During the separation of adhesions, especially those between the uterine tumour and adjacent parts, it was almost inevitable that the uterine tissue would be torn, and as the enlarged uterus in these cases was often extremely vascular, it followed that if it were lacerated in the early part of the operation, the operation would have to be done against time, since the uterine arteries could not at once be reached and tied. In the case operated on at the Paddington Infirmary great trouble had been experienced from this cause, and, even after the uterine arteries had been ligatured, the oozing from the other surfaces in the pelvis had continued. He ought, perhaps, to have packed the pelvic cavity with gauze, though this was a practice to which, under ordinary circumstances, he strongly objected. It meant leaving a great hole in the belly wall, and it was often much easier to get the gauze in than to get it out. When perimetritis co-existed with the fibroid, and the adhesions between the tumour and the intestines were extensive, the operator had to cope not only with hæmorrhage but also with shock, and thus, in these cases, there must always be considerable risk. He commented on the fact that the cases of hysterectomy which were complicated by the existence of pyosalpinx did well, although pus escaped during the removal of the

tubes, and that such cases did not necessarily require drainage. The only indication for the use of a drainage tube was free oozing of blood from the raw surfaces left by the separation of adhesions.

January 22nd and February 24th, 1900.

DISCUSSION ON "THE PATHOLOGY AND TREATMENT OF APPENDICITIS."

Mr. C. B. Lockwood observed that in the discussion which took place recently before the Society on the Prognosis in Appendicitis,* and which was introduced by Dr. H. A. Caley, this observer and some of those who spoke made use of clinical statistics to a large extent. This method of inquiry was obviously of value, but it had not hitherto carried conviction to the minds of physicians and surgeons. The reason for this was simple. Taking the 99 cases of simple appendicitis and perityphlitis, on which Dr. Caley's remarks had been based and in which the patients were said to have recovered, it was reasonable to ask how this was known to be true. A certain proportion of them must have had concretions in the appendix. The attack may have subsided, but the patients had not recovered and the appendicitis was sure to recur. It would be just as reasonable to say that a patient with stone in the bladder had recovered because, after rest in bed, an attack of cystitis had subsided. Without doubt some of the 99 cases relapsed and the patients went to other hospitals, where they were included amongst the severe cases with perforation, gangrene, or septic peritonitis. He ventured to think that appendicitis could not be correctly prognosed until it was possible to infer from the clinical symptoms the pathological changes proceeding within the appendix. He did not intend to discuss the nomenclature of diseases of the vermiform appendix: much confusion prevailed, and the classification adopted by many authors was illogical. When inflammation originated in the vermiform appendix he intended to call it appendicitis and to add terms to indicate the effects which the disease had produced, such as ulceration, perforation, gangrene, the various inflammations of the peritoneum, of the broad ligament of the uterus, of the portal vein, and so forth. The remarks which would follow were based upon the histological examination of 53 cases of appendicitis ranging from the slightest to the most acute. The specimens had been obtained from cases which he himself had operated upon and of which he knew the histories.

The first group of cases was that in which suppuration was absent. Some of these cases came under the head of "Appendicular colic." In these, so far as was known, no clear attack of appendicitis had ever occurred. They were characterised by slight tenderness on pressure over some part of the iliac fossa, by disorders of digestion, and by occasional attacks of transient pain not infrequently mistaken for renal colic.

[Regarding the normal anatomy of the appendix, Mr. Lockwood directed special attention to the lymphoid follicles and the lymphatic system of

* *Ante*, p. 47.

the appendix. The continuity of the sub-mucous and sub-peritoneal tissues through numerous gaps (*hiatus muscularis*) in the muscular coats was pointed out ; and the paths, by which inflammatory and infective inflammations of the mucosa spread to the peritoneum, were demonstrated by lantern slides.]*

In a case of appendicular colic, such as was described by Talamon, a healthy young woman suffered for 18 months from abdominal pains. During the last six months she had been unable to follow her occupation. The pain was referred to the right side of the abdomen and to the right iliac fossa. It had been diagnosed as being caused by colic. The appendix was removed. To the naked eye it appeared normal. Microscopical section, however, showed the lumen to be filled with epithelium, mucus, granules, and crystalline bodies, and crowded with diplococci, small ovoid bacilli single and in pairs, small clumps of staphylococci and streptococci in chains from four to six, and some long slender bacilli. In the micro-photograph the mucosa of the appendix was seen to be ulcerated, and these various bacteria had penetrated for perhaps half a millimetre into its substance. They might be reluctant to advise the removal of the appendix for a disease designated "appendicular colic." Such a designation conveyed but little idea of danger. But few would hesitate to remove an appendix the mucosa of which was ulcerated and invaded by bacteria.

Amongst the cases which had been included under the term "catarrhal appendicitis," until the result of the histological examination had been ascertained, the following was considered a typical case. A young man, aged 29 years, had his first attack of appendicitis at the age of 19 years. During the next 10 years he had so many attacks that he could not recall their number. They usually confined him to bed for a day or two, and prevented him from going to business. Perhaps an operation had been deferred because he suffered from mitral disease. The appendix was removed in the usual way and was not adherent. It was engorged with blood and felt very hard. It was constricted about three-quarters of an inch from the intestinal end. The micro-photograph showed the mucosa to be ulcerated and the lumen to be filled with granules, crystalline bodies, shreds of organic matter, irregular pieces of semi-transparent substance, and quantities of bacteria. These included many morphological varieties, cocci, diplococci, short ovoid bacilli single and in pairs, long slender bacilli, short thick bacilli with rounded ends, long thick bacilli, leptothrix, spores, and spore-bearing bacilli. Many of the tubular glands had shed their epithelium, which had been replaced by plugs of bacteria projecting from the lumen. In some places, these bacteria were penetrating the lymph canalicular system of the mucosa. The term "catarrhal appendicitis" did not perhaps convey to the mind a very clear sense of impending danger, but no one, who was aware of the pathological state of such an appendix as this, would hesitate for a minute in advising its removal.

Of the other pathological conditions associated with clinical signs of appendicitis he would take the effects of stenosis first. The appendix of a young woman who had had four attacks of appendicitis was removed. During the attacks the temperature rose. She had pain in the right iliac fossa, which was so severe as to cause collapse, there was constipation but

* 'Journal of Anatomy and Physiology,' 1900; 'Proceedings of the Anatomical Society,' November, 1899, p. 9.

no vomiting. Betwixt the last two attacks pressure in the iliac fossa was painful, so that it was obvious that, although appendicitis had subsided, it had not got well. The appendix was not adherent, was nearly 4 inches long, and constricted from the cæcum. A micro-photograph of the constricted part showed the mucosa beyond the constriction to possess an epithelial lining. The lumen contained pus full of streptococci. It was hardly necessary to point out the danger of this condition, but to render it clearer micro-photographs of another case were referred to. A man who had had six attacks of appendicitis was advised to have his appendix removed. The operation was deferred until after his wife had been confined. When next seen, he had no signs whatever of appendicitis, but was obviously suffering from hepatic abscess. An attempt was made to discover this abscess from behind, but without success. A second attempt was made in front. No abscess could be discovered, but inflamed glands were felt about the cæcum. The appendix was therefore removed. The micro-photograph showed that the interior contained pus full of streptococci with a few bacteria of other types. The lymph on the surface of the appendix was also full of streptococci. The patient survived this operation, but ultimately died from portal pyæmia, with a number of abscesses in the liver. The delay in this case was evidently a matter for the keenest regret, and it would never have been permitted had the pathological condition of the appendix been accurately diagnosed.

[Some of the effects of ulceration of the mucosa and subsequent infection of the appendicular wall were next illustrated by Mr. Lockwood. Also the dangers of fæcal concretion. Fæcal concretions were shown to be bacterial masses, a point which, strangely enough, had not been demonstrated by any investigator.] In continuation—

He regretted that he had not time to demonstrate, by specimens, the various steps in the formation of fæcal concretion. These specimens showed that fæcal concretions were always associated with general ulceration of the mucosa; that the concretion was not necessarily the cause of the ulceration; and that the concretion and the ulceration both owed their origin to the same cause—bacterial accumulation within the appendix. The presence of concretion in the appendix was evidence of extensive ulceration of the mucosa and of bacterial invasion. A large concretion was in itself a mechanical peril when, owing to the application of cold to the surface of the body, the appendix became engorged. It might cause a perforation of the appendix in its vicinity, or gangrene of the part beyond. If it were true that many of the cases of appendicitis which were said to have recovered did, as a matter of fact, contain concretions it was easy to imagine their peril, and the absurdity of including them in statistics of recoveries.

Of the so-called process of natural cure the accounts given by authors were fanciful. It was impossible to say when the so-called process of natural cure had been achieved. The appendix had been removed from a man who had had at least 20 attacks of appendicitis. He had twice been in large London hospitals, once under a surgeon and once under a physician. As they sent him out again without operation it was reasonable to suppose that they anticipated that the natural process of cure would eventually take place. The appendix was removed. A histological examination showed that the mucosa had been destroyed by ulceration and that the lumen was occupied by a concretion which seemed calculated to prevent any supposititious natural cure from taking place. An appendix which had once been inflamed might inflame again after

long intervals. An appendix was recently removed for repeated and violent attacks of appendicitis which had recurred after nine years' quiescence. In another case, the patient had a violent attack of appendicitis, then an interval of 15 years of quiescence, followed by another violent attack, after which the appendix was removed. It did not seem reasonable to lay down rules as to how many attacks should be suffered before the appendix was removed. He was inclined to wait for some time after the first attack of non-suppurative appendicitis to see what supervened. But on several occasions the appendix had been removed after the first attack, because it was still painful and could be felt. Under such circumstances the general health of the patient was seldom good, and the functions of the alimentary tract were improperly performed ; indigestion, flatulence, constipation, and diarrhœa being frequent accompaniments. After a second attack of non-suppurative appendicitis, most patients preferred to part with an organ which appeared to be of no physiological importance, and was an actual source of danger.

As to the question of diagnosis, it was usually possible to say that the appendix was inflamed, but hitherto the histology of appendicitis had hardly been sufficiently investigated for anybody to surmise, with accuracy, the actual pathological state of the appendix. The usual sequence of events, as shown by the histological examination of the 53 specimens, was destruction of the epithelial lining of the lumen, ulceration of the mucosa, bacterial invasion of the mucosa and submucosa, and extension of the inflammation through the muscular gaps to the peritoneum. This process might be complicated by the formation of cicatricial strictures, by inflammatory obliteration of the whole lumen after removal of the mucosa, by the presence of fæcal concretions, by perforation, by gangrene, by inflammation of the appendicular lymphatics, and by portal pyæmia. It was also important to note that, as Clado was the first to point out, the inflammation in females not infrequently spread along the lymphatics to the right broad ligament of the uterus, so that appendicitis was sometimes mistaken for inflammation of the ovary or Fallopian tube. In three cases of this kind, operations had been performed. In addition it was important to observe that appendicitis sometimes occurred as a complication of other diseases. Primary tuberculosis of the appendix was probably a rare disease ; although every specimen had been carefully examined for tubercle it had only once been discovered. But on one occasion, in operating for the removal of an inflamed appendix, general tuberculosis of the peritoneum was discovered, the inflammation of the appendix being a complication of that disease. In another case of appendicitis, a hard mass was discovered at the base of the appendix. When a portion of this was submitted to histological examination it was found to be cancerous. The cancer, which involved the ileo-cæcal valve, was removed and the intestine was united by circular enterorraphy. The patient recovered but ultimately died from a recurrence of the cancer in the pelvis. As regarded cases of appendicitis in which perforation or gangrene had occurred, there could be little doubt as to the propriety of evacuating and draining the abscess as soon as diagnosis could be effected. The cases showed that the safety of the patient depended mainly upon the stage at which the operation was performed, and the position which the appendix occupied. When the appendix was beneath the cæcum and in the iliac fossa, the chances of the abscess being localised were fair ; likewise when the appendix lay to the outer side of the cæcum and colon. But, in females especially, the

appendix often hung over the brim of the pelvis into Douglas's pouch ; when such an appendix perforated or became gangrenous the danger was vastly increased. Moreover, collections of pus at the bottom of Douglas's pouch were most difficult to drain.

As regarded the operation, he advocated the oblique incision parallel to the outer half of Poupart's ligament, because it could be extended to reach an appendix lying outside the cæcum, or adherent within the pelvis. The wound seldom exceeded 3 inches in length ; such a length as 2 inches or $2\frac{1}{2}$ inches was very common. The layers of the abdominal wall were split and not cut across. In non-suppurative cases, the appendix might be most difficult to discover. It might be laid down, as an axiom, that the organ was never absent except as the result of disease. Cases were mentioned in which the organ had only been discovered with the greatest difficulty, because it was hidden away in either the subcæcal or the ileo-cæcal retro-peritoneal fossa. Doubtless, surgeons who had failed to find the appendix had not infrequently been deceived by this abnormality. In cases complicated with suppuration and peritonitis a determined endeavour should be made to remove the appendix. It was worth while to run considerable risks rather than to leave behind an appendix which was profoundly septic and which might contain concretions. In some cases, in which the abscess had merely been opened and drained and the appendix had been left behind, subsequent attacks of appendicitis had been known to occur. In others, a septic sinus had persisted requiring a formidable and often fatal operation for the removal of the appendix. In one such fatal case, the sinus led to a perforated appendix which contained a large concretion. Nevertheless, the patient's condition sometimes forbade an attempt to remove the appendix. A case of this kind was under observation in which the original appendicular abscess had been followed, at intervals, by three others of the most formidable and dangerous type. As regarded the closure of the wounds after the removal of the appendix, in cases without suppuration, it had been found inadvisable to insert buried sutures of silk, because infection was likely to occur when the lumen of the appendix was cut across. Good results had been obtained with a single row of fishing-gut sutures.

Operations for the removal of the appendix varied greatly in their gravity. The simple operation was comparable to the radical cure of non-strangulated inguinal hernia. But occasionally after violent appendicitis, the most formidable adhesions were met with which might test all the courage and dexterity of the operator before they were overcome. In his own operations for the removal of the vermiform appendix, in which suppuration was absent, the patients recovered more quickly when they were kept lying upon their side and were quickly put on solid food. In cases of appendicitis, with perforation or gangrene and acute abscess, the after-treatment required the greatest skill and watchfulness.

Dr. W. H. Allchin commented upon the change that had taken place, during the last few years, in the position of the physician in respect to the treatment of this disease. So marked was this change, indeed, that really the surgeon ought now to take precedence. This alteration in position was largely dependent upon the increased knowledge which had been gained within the last few years as to the exact nature and pathology of the disease, although much still remained to be ascertained. He referred to the admirable improvements in the surgical methods of

treatment which had resulted in the brilliant successes which surgeons had been able to achieve in the treatment of this and other abdominal conditions. It was satisfactory that one who had contributed so much as the author of the opening paper towards placing the treatment of the disease upon the very satisfactory basis which it occupied at the present time, should have given them the benefit of his observations, which were of exceeding value in connection with the pathology of the affection. Confining himself to the subject of treatment, which he had understood from the President was to be the main subject of discussion, he observed that in those years—not so long ago—when their knowledge of the disease might be described as exceedingly vague and mostly incorrect, and when the peritoneum and other serous membranes in the body were looked upon as structures not to be touched by the surgeon, if they could possibly be avoided, the treatment of the disease to a large extent could be summed up in what they might describe as an attitude of ill-regulated expectancy in which the surgeon had little, if any, part. At the present time it was very much the other way; there were some, indeed, who held that the physician had nothing whatever to do with the treatment of this malady and that cases should at once pass under the surgeon's care. With every admiration for what had been done in the domain of surgery, he did not think that their present knowledge justified such a view. When a patient came to them, and they were in doubt as to the exact condition of the appendix, when there were certain clinical symptoms and certain physical signs, they still had to exercise much imagination as to the exact condition they were called upon to treat. These various cases seem to fall in one or other of the following groups, each of which calls for somewhat different treatment. There were four groups which were fairly distinguishable from each other, which might pass from one into the other, but which are convenient to bear in mind in considering any individual case:—(1) The acute, severe, or fulminant, occurring in persons previously in apparently good health, who promptly fell into a condition of the gravest danger and might die in a few hours. (2) Cases still acute, but less acute than the preceding cases, which lasted some days after evidence of suppuration appeared. (3) This group comprised the cases that usually came into the hands of the physician. They, too, were to some extent acute, but less so than the preceding groups; in his experience, at any rate, they were more frequently met with in females than in males, and they lasted 12 to 14 days. There was pain, tenderness in the right iliac region with some swelling and vomiting, and considerable general disturbance which, however, varied greatly in amount; some elevation of temperature, though this was not invariably present, and a moderately quick pulse. This was the condition of the great majority of cases that found their way into the medical wards and, he believed, formed the greater number of cases to which the term appendicitis would be applied. These cases ran their course to what was apparently a cure, though this, he admitted, could not be proved. Such cases, he maintained, prominently called for medical treatment, and he had yet to be persuaded that it was desirable, or even justifiable, to insist that an operation should at once be performed. He called special attention to the great variation from day to day in the general condition of the symptoms and in the size of the swelling. (4) Cases which recurred or relapsed. Putting the last group aside for a moment, they had to relegate each case, as it came before them, to its proper group. Obviously in the fulminant cases, the surgeon alone was of avail as also in the second

group when there were obvious signs of suppuration having taken place. In the third group he hesitated to regard an operation as necessary. He admitted the difficulty in that a particular case might pass on to a condition of suppuration or might apparently get well only to relapse a short time afterwards. Of the various symptoms to which he attached the greatest importance as indicating the severity of a given case, the most significant was an increase in the pulse rate. He did not think that any one of the other symptoms—pain, temperature, and certainly not the swelling, was of anything like the same value in respect of prognosis. Judging from his own experience these cases would probably get well in from 12 to 14 days. Apart from being in bed, the next thing was to have the bowels relieved. Hitherto he had been satisfied in great measure with achieving this result by simple means every day or every other day, and this might be supplemented by the administration of salines if the history showed a tendency to constipation. Although he knew that his view would be regarded in some quarters as heretical, he was constrained to say that he had found great advantage attend the administration of small quantities of opium. He was aware that it had been accused of masking the symptoms, and that argument was so plausible that it appeared to have carried greater weight than it perhaps deserved. In small doses, he contended, opium was distinctly beneficial, but he admitted that if given in too large doses it might, of course, mask the symptoms. Under the influence of these small doses, he believed that it was easier to appreciate the import of particular symptoms—the pulse, for example—than when the patient was disturbed and restless. Another measure was hot fomentations when they could be borne. These measures constituted the medical treatment. With regard to the relapsing cases, these, he admitted, offered the greatest possible difficulty. The chances were that very great good might be done by the surgeon, but he would be loath to accept his intervention as indispensable merely on the strength of statistics, which were exceedingly difficult to apply to any individual case. Still more would he deprecate many of the dogmatic rules laid down by some: that the patient should be allowed to remain so many days, or that the attack should recur so many times before calling in the surgeon. The proportion of these relapsing cases which had benefited very considerably by medical treatment, or treatment short of operation, was considerable, but he admitted that it was much more difficult to decide the question of what method of treatment to adopt in this particular group of cases than in any of the others.

Mr. A. Barker observed that so many points of great interest bearing on the pathology and treatment of appendicitis had been raised by the author and the preceding speaker that it was difficult to know where to begin to comment on them. He was particularly interested by the admirable way in which the author had traced the septic organisms into the lymphatic system, thereby providing an explanation of a very difficult class of cases in which, with all the symptoms of appendicitis leading to operation, to the naked eye the appendix appeared normal—where it was not inflamed, where there were no adhesions, and yet where an abscess was found behind the cæcum. This had an important bearing also upon other cases beginning with appendicitis, where there was extensive cellulitis in the loin, reaching in some cases to the diaphragm, and giving rise, possibly, to sub-phrenic abscess. If the affection were in the

absorbents behind the cæcum, they might well have infection of the lymphatics actually extending to those of the sub-phrenic space. He had seen more than one instance of that, and the explanation which he had imagined for himself was based upon a speculation that the lymphatics were the channels of infection, rather than upon an actual demonstration of their course in this region, such as Mr. Lockwood had just given them. Referring to the controlling power of the serous covering of the abdomen —whether parietal, visceral, or omental—over the ravages of bacteria, he thought they did not give the peritoneum its due in this respect. It does not follow that, if a few septic organisms get into the abdomen, they must, therefore, needs spread widely. Those who adopt the opposite view, he thought, assumed too much, for he believed that the peritoneum had a vast power of controlling these organisms, of which they ought to take account when operating. If they believed that septic organisms, having infected the tags of omentum around the diseased appendix, were going to spread, and that the controlling power of the peritoneum—what they might call its bactericidal power—was not going to restrain them, they ought to take away the omentum widely, and go a very great length in washing out the peritoneum. His own experience had led him to the conclusion that the abdominal cavity was very largely able to take care of itself. He only took away very small parts of the omentum which were obviously gangrenous or deeply inflamed ; in fact, he had been jealous to preserve it in every way. He had also grown more and more conservative in dealing with the peritoneal cavity, restricting his operations to the most limited area, and, even in widespread suppuration, avoiding any attempt to clean the whole abdomen. He was glad to hear Dr. Allchin put in a plea for opium in these cases, although in this respect he differed from the views of many surgeons. He held that there was a good deal for physicians to do in these cases, and if they could assist the surgeons to discriminate, to know in which cases they ought to operate, and what cases would be best left to themselves, they would confer a great favour on them. It must be admitted, however, that they were constantly being tripped up by these cases. Cases which appeared likely to recover startled them by sudden secondary outbursts entailing the greatest risks, while others which appeared grave subsided and lived in peace for years. He referred to the case of a gentleman, aged 28, who came to him after having had an attack of appendicitis three weeks before. His doctor had advised him to have the appendix removed in the interval. There was nothing beyond a suspicion of swelling in the groin, and as the patient was disinclined to have anything done, but wanted to go hunting, he did not strongly urge operation, but protested against his hunting, and advised him to take a further period of rest. Two or three weeks later he was asked to open the abdomen, and when he came down to the cæcum a little piece of adherent omentum was found. On turning it up, he discovered a concretion, as large as the distal joint of his thumb, lying loose, having ulcerated through the appendix, and even eroded a hole in the ileum, yet there was no pus. Mr. Barker added that if this patient's doctor had not given him better advice than he himself had given, the probability was that he would have gone hunting with this body loose in his abdomen, and the almost inevitable result would have been the setting up of general peritonitis. As it was, a timely operation, including the removal of the remains of the appendix and suture of the two holes, was followed by an uninterrupted recovery.

Dr. F. J. Smith said that he had been asked some time ago by Mr. Treves to look up all the cases of appendicitis in the case books of the London Hospital. In this research he had been much struck by the fact that, just as in perforation of the stomach, there was often such a history as the following :—A man going to work was suddenly taken ill, was carried home, and was operated upon within 24 hours, when they found a perforated and gangrenous appendix. So far as the records went there had been in most instances no previous pain, yet the appendix was gangrenous within 24 hours. He looked upon the ulceration of the mucous membrane as the removal of the first line of defence, and this ulceration, he supposed, was frequently in itself painless. He remarked, speaking as a pathologist, that in removing the intestines *post mortem* they were apt to be torn ; and it had occurred to him to attempt the cleansing of the abdomen, but, even with a hose of water under great pressure, he had never yet succeeded in doing it, and he doubted whether any surgeon could do it on the operating table. He thought, therefore, that the less there was of washing out the peritoneum the better. Dr. Allchin had made use of the expression "obvious signs of suppuration." When there were "obvious signs," there was no room for the physician ; but the difficulty was how to tell when there was pus. He admitted that in nine cases out of ten the presence of pus would be obvious even to a third year's student. It was in Dr. Allchin's two intermediate classes that one required the keenest eye for every little change, in order to estimate whether pus were present or not. He agreed that the pulse was probably the most valuable sign, but sweating at night was another to which he also attached considerable importance. He admitted, however, that they could not wait many nights, possibly not a single night, to establish this important point. He thought that salicylate of soda and ice bags were of assistance in cases belonging to the second and third groups. He added that he had very carefully noted the exact anatomical position of the appendix for many years, and he was inclined to believe that it was most commonly found tucked up behind the colon. This might not be very important, except, perhaps, to suggest to the surgeon the advisability of cutting down at a point where he would be able to get at the back of the cæcum and ascending colon. If the appendix hung into the pelvis, there might be interference with micturition, and pain on completing the act when an inflamed appendix was in contact with the bladder. This sign, he thought, might be useful for diagnosis in certain cases.

Adjourned Discussion, February 26th, 1900.

Mr. Marmaduke Sheild commended Mr. Lockwood's plan of correlating clinical symptoms with pathological conditions, and regarded his work on the bacteriology of appendicitis as especially valuable, showing, as it did, that bacterial invasion of the coats of the appendix was at the bottom of many of these attacks. In reference to the non-operative treatment, they must never expect all medical men to agree. He held strongly that only in slight cases was the non-operative treatment justifiable. No doubt cases ushered in with great severity might survive under this treatment, but, personally, he would not accept such a responsibility. The indications for operation in appendicitis were in direct proportion to the severity of the symptoms ; and the more acute the case,

the greater the indication afforded by severe pain, meteorism, rapid pulse, and vomiting. In mild cases they must watch, empty the bowels by enemata, and avoid the use of opium, except to subdue pain. In large doses this drug masked the symptoms. He insisted upon the impossibility of being actually sure of the pathological condition present, and thought that those who habitually adopted the expectant treatment, if they could look inside and see what that condition was, would shudder at the risks they were running. He referred to the case of a girl, aged 16, with a first attack. He saw her on the following morning; the symptoms were slight—a little pain in the iliac fossa, and the only disquieting condition was the pulse, which was over 120. There was a great difference of opinion as to the advisability of exploring the appendix, but, on doing so, he found it enlarged and inflamed, and on removing it a quantity of pus escaped. Instances of this kind showed how difficult it was to appreciate the symptoms in these cases. In reference to the operative treatment they were all agreed that in very severe cases, in fulminating appendicitis and recurrent cases, operation was necessary. It was only in the slight cases that the difficulty arose. The one point he laid stress upon, and which had not perhaps been sufficiently urged, was the absence of induration or of a lump in the iliac fossa. In cases of doubt, if a mass could be felt, they might more safely wait for a few hours, or days, than if nothing were perceptible except rigidity and local pain. Cases marked by local rigidity and tenderness were often very grave. Consequently, whenever, even in slight cases, the symptoms were progressing unfavourably, and especially if the pulse were becoming rapid, an operation was indicated. The pulse should be taken every hour. He had never had occasion to regret having adopted this line of conduct. There was not much to be said about the actual nature of the operation. He advised as small an incision as possible, and in recent cases at the first or second attack a 1½-inch or 2-inch incision was ample. He always adopted the plan of splitting the muscles, and he asked what was the author's practice in this respect. Where there had been many attacks with dense adhesions, however, a free incision might be required. In such cases, much of the difficulty that was experienced in removing the appendix was due to the operator trying to get it away through a small incision. In regard to the closure of the wound, he had had occasional trouble with buried sutures when he used silk, but since he had adopted kangaroo tendon for suturing the muscles no trouble had arisen. With regard to after treatment in severe cases with peritonitis and an area of paralysed gut, the great thing was to get a free action of the bowel as soon as possible. With this object in view, he gave the patient powerful salines by the mouth, and turpentine enemata. When flatus was passed freely the patient was probably out of danger. He concluded by observing that they must remember that no two men would probably agree as to the treatment of these cases, and that they were eminently cases for consultation between physician and surgeon; but he was convinced that when the symptoms progressed and were severe, operative treatment should not be delayed. He had seen several tragedies, mostly in private practice, which were chiefly due to ill-judged expectant treatment. The better the social position of the patient, the less chance he had of being relieved of a damaged appendix. He had made up his mind never to sanction expectant treatment in these cases, unless the symptoms were slight and showing a tendency to improve.

Dr. H. A. Caley agreed that the non-operative treatment applied to the milder cases—the only question being as to the relative proportion of these cases. Mr. Lockwood had referred to some figures in a paper which he, the speaker, had read before the Society at a previous meeting, and had criticised the use of the word "recovery" in connection with attacks of appendicitis. In his own series of 200 cases, he had given 99 as recoveries, but he had made it clear that he used the term "recovery" in the limited sense of "recovery from the attack." Admitting that statistics in connection with appendicitis were more than usually untrustworthy, they nevertheless served to illustrate certain points. With regard to these cases the recovery of which had been questioned, he pointed out that they were consecutive cases extending over five years, and among them about 30 per cent. subsequently relapsed. It might, of course, be said some of the other cases may have relapsed and been treated at another hospital, or may have died; but allowing for sources of error, such figures were sufficiently correct and served to illustrate the considerable proportion of mild attacks which underwent recovery, and in which no grave results subsequently occurred. In that connection Mr. Lockwood had stated that, no doubt, among the cases recovered, concretions were present in a certain proportion, and he had cited the parallel of vesical calculus, instancing the case of recovery from cystitis associated with calculus, and observing that this would not necessarily imply that the calculus was recovered from. If one allowed that parallel it must be remembered that just as there were many cases of cystitis not associated with calculus, so there were many cases of appendicitis not associated with concretions. The proportion of concretions in recurrent cases was less considerable than other lesions, such as stenosis, chronic inflammation, &c. Thus, on pathological grounds, they had a definite and considerable group of cases, not associated with ulceration, gangrene, or other of the more serious lesions of the appendix, which, as a rule, subsided, and, if they relapsed, they did not usually relapse into a more severe form. The whole point of his own paper had been to endeavour to establish that correlation between the pathological conditions and the clinical manifestations as seen at the bedside, which was the only sound basis of prognosis. With regard to treatment, he remarked that Dr. Allchin, in introducing the subject from the medical side, advocated operation in three classes of cases: the acute fulminating cases, those associated with suppuration, and relapsing cases. Presumably they were all agreed as to these three groups, but the difficulty arose with regard to another group of cases—some acute cases seen early, in which the initial symptoms were severe, and the general condition suggested a grave lesion, yet in which, when first seen, it was practically impossible to tell whether the case would ultimately assume a severe form, or whether the graver symptoms would subside. These cases, in the speaker's opinion, constituted a fourth group in which early operation was certainly indicated. He considered that, if all cases corresponding with the above four groups were generally recognised as requiring operation, it would leave them with a much clearer ground for another and large series of cases which do quite well without operative treatment. With regard to the relative significance of the different symptoms as indicating operation, he had entered at some length into that question in his paper. He remarked that most speakers had laid great stress upon a frequent pulse rate, *e.g.*, 120, and in this he concurred; but the converse of that did not hold good, for some of the very severe cases might have a not very

frequent pulse, particularly at first. The pulse, then, must be considered in conjunction with other conditions—respiration, temperature, and especially the abdominal condition. With regard to the use of opium, he supposed that no one now advocated its continued administration in large doses as a routine measure, but he thought it must be conceded that it rendered great service if used with discrimination. Two important provisos should be borne in mind ; first, that it should never be prescribed until the question of operation had been decided, and secondly, it should only be given to meet some special indications for the relief of pain or restlessness, and then not continuously, as against rule. Nothing could be more distressing than to see a patient, who had been kept under opium for two or three days, with a gradually progressing lesion which should have been operated upon before, but this was the abuse of opium and not its legitimate use.

Mr. Harrison Cripps entirely agreed with Mr. Sheild's opening remarks. With regard to treatment, they must clearly divide the cases into two distinct categories : first, the comparatively mild attacks which came on one after another—the so-called intermittent appendicitis ; and, secondly, the far more severe attacks of the fulminating type. In respect to the first group, if the patient had but a single attack, and that not very severe, it was just possible that the cause was accidental, and, if seen when the attack had subsided, he would not advise operation. If, however, it were the second or third attack, the case was entirely altered, for the recurrence showed that the first attack was not accidental, but was due to some abiding condition that unhesitatingly called for operation. If called to see a patient who had had two or three attacks, even when there was no pain or tenderness in the intervals, he would still strongly urge operation, as he considered that the patient would otherwise be carrying about a dynamite shell in his inside, which might, at any moment, explode and put his life in danger. The risk to life from operation was very small, much smaller indeed than the danger of leaving the patient in that condition. His own experience of cases operated upon during the quiescent period was that the mortality was very small, in fact, personally, he had never had a fatal case, and Mr. Treves, with 130 or 140 cases operated on in the quiescent period, had only had one death. The absence of pain, tenderness, or swelling in the region of the appendix in a patient who had had three or four attacks, was sometimes given as a reason for not operating, on the ground that if anything were wrong with the appendix something would be perceptible to the fingers, but this was not the case. The presence of pain or swelling depended greatly upon the situation of the appendix. If situated at the side of the cæcum, in a thin patient, then it would be perceptible ; but, in a moderately stout person, or when situated behind the cæcum, nothing might be felt. He referred to a case at St. Bartholomew's Hospital, which was supposed to be one of inflamed ovary. A tender mass, the size of a tennis ball, could be felt at the bottom of Douglas's pouch. When he operated he found that this was the head of the appendix surrounded by a ball of omentum, which was adherent, and had dragged it down—its length, in this stretched condition, being estimated as at least 9 inches. Though he was satisfied as to what was the right treatment in the recurrent cases, he was not so sure as to what was the best course in the fulminating attacks. Speaking generally, when a patient was suffering from an acute attack of appen-

dicitis, he was in an unfavourable condition for surgical interference; the intestines were distended, there was more or less vomiting, so that he took the anæsthetic badly and suffered from shock. In spite of this, in all cases of acute appendicitis, he would operate with the least possible delay; for, although the risk was great, the danger of leaving them alone was greater still. He made, however, one exception, viz., cases which had been going on for two or three days in which there was a considerable mass of inflammatory tissue lying over the region, like a plate or saucer, with œdema, showing an abscess was about to form. If left alone, it would be seen where the abscess was going to point; but if this brawny mass were cut through before the matter had formed the surgeon would not know where he was, and might have great difficulty in finding the appendix. With respect to the size of the incision, he himself generally preferred one of fair size so as to enable him to see what he was doing, but he pointed out that it was important not to divide the aponeurosis of the external oblique, which should be split, but he preferred to cut through the deeper muscles. He abominated every form of buried sutures, and very good results could be obtained with sutures going through all the structures. In his earlier cases he used to order a pad over the wound, but latterly he had entirely abandoned that practice, as he found that the pad was really not only of no use, but that it was injurious and tended to atrophy the muscles.

Dr. Hunter observed that there were cases which recovered to the extent of 70 per cent. without any intervention. The surgeon, being called in only to severe cases requiring to be operated upon, naturally formed a different opinion from that of the physician who saw these milder cases. As a pathologist he had occasion to meet with both classes of cases. He was glad that the subject had been dealt with from the point of view of the pathology and treatment of the disease, because the disease was one in which the treatment was very largely influenced by their views of the pathology. Mr. Lockwood had done good service by directing attention to the views and conclusions which he had laid before the Society. It was customary to speak of three types: the catarrhal, the ulcerative with concretions, and the fulminating variety with necrosis of the appendix and peritonitis. The distinction was usually drawn between the catarrhal form on the one hand and the fulminating on the other. His own view, as a pathologist, was that no such distinction could be drawn; the catarrhal form, in his opinion, being as much an infective condition as the fulminating. This was important, because it put all the cases of appendicitis into one large group. If they took simple catarrh of any other part of the intestinal tract, this had nothing in common with catarrh of the appendix, which was an infective condition involving the total thickness of the walls, and possibly extending to the peritoneum outside. He called attention to a number of specimens he had prepared illustrating catarrh of the appendix, and the various conditions met with in recurrent cases. Dealing first with the operative treatment, he said the operative cases might be distinguished very much as Dr. Caley had suggested, viz., (1) fulminating appendicitis which must be operated on at once irrespective of the trouble it might give to the surgeon, and (2) ulcerative appendicitis with abscess formation. He said he had very strong views as to the time at which operative measures should be resorted to. The slightest rise of temperature—99, 100, or 101—seven or eight days after the acute symptoms

had run their course was, to his mind, an almost certain indication of the formation of pus in connection with appendicitis. It was his practice to recommend that the surgeon be called in under these circumstances. He had sometimes been met by the statement that the surgeon must wait until a certain time had elapsed, or until a sufficient amount of pus had formed before operating. That was, of course, a question for the surgeon to decide, but personally he felt that the operation ought to be performed within 24 or 48 hours of the rise of temperature. Then there was the third group of cases, not perhaps sufficiently recognised by surgeons, the 50 to 70 per cent. of cases which recovered without operative treatment. The matter was not as simple as surgeons represented it. In hospital practice he agreed that the chances of a poor patient were infinitely greater, in respect of recovery, than those of better class patients outside, with whom considerations of fee for calling in the surgeon's assistance, often at a distance, had necessarily very great weight. Under these circumstances the physician was influenced by two considerations. He knew by practical experience that about 50 per cent. of the cases would recover without operation, the only point being to consider what proportion of them showed dangerous symptoms requiring a surgeon to be called in. This was one of the most difficult problems in connection with medical practice. He was influenced greatly in this respect by his views as to the pathology, and looked upon every case as potentially severe. He presumed everyone agreed that it was an infective condition. Under these circumstances the result was, in any case, a question of the dose of the poison and the resistance of the patient, two varying factors which they were not in a position to estimate with any approach to accuracy. He, therefore, did not think any hard and fast rule could be laid down as to the best course to adopt. He insisted that there were no rules with regard to pulse, temperature, or abdominal symptoms which would enable them to divine when a simple catarrhal appendicitis would become suddenly and violently destructive. With regard to medical treatment, it was formerly held that the symptoms were due to some inflammatory infection of the colon or cæcum, and this older view still influenced treatment. It was right enough to clear out the lower bowel, but some physicians went so far as to repeat this process every night. He did not think the slightest risk attended the presence in the cæcum or colon of a small quantity of fæces, and, on the whole, the quieter they kept the intestinal tract the better, in his opinion, for the patient. This was evident in the specimens he showed illustrating the conditions met with in cases of appendicitis. There was nothing they could do by enemata that would influence, in the slightest degree, the pathological condition that was going on. In only one of his 12 specimens was a fæcal calculus found, and that was one in which there was no appendicitis. The medical treatment was summed up by perfect quiescence, small doses of opium short of producing narcosis, plus the administration of an intestinal antiseptic such as calomel and opium.

Mr. McAdam Eccles said that he desired to endorse the remarks that had just fallen from Dr. Hunter, since he had been closely connected with him over a case of acute appendicitis, namely, that one which had occurred in his, Mr. Eccles's, own person. He thought that the successful treatment, and particularly the successful surgical treatment of inflammation of the vermiform appendix depended chiefly upon two factors.

One was the violence of the bacteria, bacilli, and cocci that invaded the organ, and the other was the early diagnosis of the condition. If by removal of the appendix in the early stages of an acute attack, say within the first 36 hours, they could be sure of removing the whole of the affected area together with all the bacteria, then he thought that early operation was not only justifiable, but strongly called for. But, unfortunately, the patient was often not even seen by a medical man until the bacteria had invaded so far that such a happy result was well nigh impossible. Mr. Lockwood had most conclusively shown at the previous meeting how rapid and extensive were the inroads of bacteria, and Mr. Eccles's own observations had brought him to believe the absolute truth of these statements. He ventured to think that many of these cases were best left for the patient's own tissues to deal with, for it was an undoubted fact that they were capable of doing so satisfactorily, and that not infrequently. He was convinced of the fact that the so-called catarrhal appendicitis was always the outcome of bacterial invasion, and felt that, although he deemed it unwise to touch the case during the actual attack itself, it should be submitted to operation when the inflammation had subsided. That other class of case in which the temperature falls, as in his own case, within 48 hours of the onset of symptoms, and the patient seems to be rid of his trouble, might well be left for several days, and then if there was any rise of temperature, as Dr. Hunter had pointed out, it indicated the formation of pus, which should be immediately evacuated—an operation of comparative safety at this stage. There was a possibility that a case of this kind might have been spared the onset of suppuration if an operation had been undertaken at quite an early stage, but in his own case, and in many another, the early operation had been out of the question from the circumstances of the case, and then he was sure that it was best to wait for a time until definite evidence of pus was present. He was at one with the other speakers who had urged operation in all cases of fulminating appendicitis and in those instances where the inflammation had recurred. A person was not safe who possessed an appendix in which inflammation might light up at any moment.

Mr. Mansell Moullin said he understood that appendicitis in every case was infective, though he had met with one or two cases in which he had been unable to prove infection. In one there was a grape stone in the appendix, and he could not find any breach of epithelium. That, of course, did not enable him to assert that there were no bacteria. He was quite ready to accept Dr. Allchin's classification, though it was not altogether satisfactory because it took no account of the pathological changes. They were all agreed with regard to the fulminating and the recurrent cases. The next class of cases, that in which suppuration was present, formed a much larger proportion than was generally supposed. He believed that the sudden subsidence of the symptoms, which occurred in many cases, was due to the bursting of an abscess into some portion of the intestine ; and in operating on recurrent cases he had often found remains of dried-up pus where none had been expected. No one doubted that these cases should be operated upon, the only question being when ? He thought it would be much better as a preventive measure to operate at once before suppuration had set in. He pointed out that they had to deal with an acute infective inflammation of the appendix, an organ lying free in the peritoneal cavity. They had absolutely no clue to the

pathological changes that were going on, and no means of checking or controlling them except the limited benefit of opium and salicylate of soda. An exploratory operation performed at a very early stage, say within 24 or 48 hours, by means of a 2-inch incision in the right iliac fossa, was a perfectly safe procedure. The alternative was to await events without in the least being able to control them. He might be met by the argument that it was almost impossible in these cases to tell beforehand the actual condition of the appendix. With that he agreed, adding, however, that in nearly every case it was possible to say within 48 hours whether it was going to be a severe attack or not. Absence of symptoms was often quoted as rendering these cases exceedingly difficult, but he did not attach the least importance to this. If at the end of 24 hours the pulse went on increasing in rapidity, that alone was a sufficient justification for operation. In regard to the other symptoms—if the vomiting, tenderness, or pain increased, he thought they were bound to make an incision. He had not yet met with a severe case in which the pulse was infrequent. Discussing the cases of so-called apparent cure—cases in which there was a considerable effusion in the right iliac fossa and which nearly all got well or left the hospital without operation—he pointed out that the effusion was not due to fæcal accumulation, but to exudation of lymph. Was it conceivable, he asked, that this could ever be absorbed so thoroughly as to leave the appendix perfectly free from adhesions and constrictions? He believed that the great bulk of recurrences arose from this cause. He could not agree with Dr. Allchin that these recurrent cases with a large amount of effusion should not be operated upon. He did not wish to take these cases away from the physicians, but he thought the surgeon ought to be called earlier in acute cases, both in private and in hospital practice. He believed that the mortality from operation for appendicitis would be considerably diminished if it were performed at an earlier stage than was usually the case.

Mr. Lynn Thomas (Cardiff) said he had operated 41 times for appendicitis, with two deaths; one of which was due to pylephlebitis from simple abscess consequent to appendicitis or perityphlitis, and the other being a case of abscess localised in and around the colon and small intestine with symptoms of septicæmia. He thought the cases in which there had been mistakes in diagnosis were very instructive. He mentioned one case sent in as typhoid fever, which he had seen three weeks after, and had then diagnosed as perityphlitis. He operated, and it turned out to be gonorrhœal salpingitis, the right tube being attached to the end of the cæcum. Another case presenting the characteristic sausage-shaped tumour, proved to be an abscess of the right ovary with hydrosalpinx, to which the appendix was attached. He also referred to the case of a young lady who had come down from London to be operated upon, but the appendix, when removed, proved to be healthy. She remained free from the symptoms (appendicular colic) for two months, and then they recurred. She then returned to London and underwent six weeks of the Weir Mitchell treatment, under which the symptoms improved. His fourth case was one of perforation of the duodenum. He approved of buried silk sutures, and had used them for his last 13 cases of appendicectomy. These had all healed up without any trace of irritation, which he thought was greatly due to the wearing of gloves during the operations. He used buried sutures because he had

seen such a number of herniæ following the ordinary method of simply closing the wound *en masse*. He thought if they collected statistics of cases in which the wound was closed by a series of buried sutures, the results would be found to be greatly superior to those of the other method.

Mr. JAMES BERRY agreed that operation ought to be the rule in chronic relapsing cases because they rarely recovered completely, and the operation itself was of very little moment, whilst it saved an enormous amount of suffering. It was important that the incision should be made through the sheath of the rectus and not outside, in order to diminish the risk of hernia. He approved of the use of silk and buried sutures. He had done 15 operations in this way, and none had been followed by hernia. In regard to the very acute or fulminating cases, the operation could hardly be done too soon. The incision should be a free one, and all pus or septic matter ought to be sponged out, and not washed out, a practice to which he was greatly opposed; in fact, he did not believe the irrigation method could be supported by any extensive series of cases. With regard to the very numerous cases which were not fulminating and which were not chronic, it was difficult and, perhaps, impossible to lay down any definite rule. He believed that, as a rule, early operation with removal of the appendix did more harm than good in these cases, since the operation was attended by a considerable mortality. On the other hand, the delay of a few days permitted, in the great majority of cases, the performance of a comparatively safe operation—the local opening of an abscess through the adhesions. Many cases which began very acutely ended in resolution without any operation at all. Mr. Treves had spoken very strongly against operating very early in acute appendicitis. Mr. Berry remarked on the curious fact that when a surgeon was unfortunate enough to become himself the subject of acute appendicitis, he nearly always sent, in the first instance, for a physician and not for a surgeon. He knew of many surgeons in London who had suffered in this way, but had not yet heard of one who had been operated upon in the acute stage.

The PRESIDENT insisted upon the importance of studying the treatment of appendicitis from the clinical side as well as the pathological. He agreed that many of the patients who were treated in a hospital had a far better chance than persons in a higher social position, for various reasons, financial and other. At the present day, no intelligent practitioner would advocate violent medicinal treatment in these cases; and, if the physician did not at once send them to the surgeon, he was very cautious as to what he did in the way of active treatment. He saw no objection to Dr. Hunter's suggestion to give small doses of calomel with opium. He thought that in cases of appendicitis there should, if practicable, be a consultation between the physician, the surgeon, and the general practitioner as to the measures to be adopted. It sometimes happened that the surgeon would not do what seemed indicated in the way of operative treatment according to the more advanced modern ideas. He had at present under his care a patient who had just recovered from a second attack of undoubted appendicitis. He presumed that the general feeling among surgeons was that the recurrent cases ought to be operated upon when things were quiescent, but his colleague declined to interfere. The question suggested itself whether it would not be a good

plan, from the patients' point of view, to encourage the practice of removing the appendix in early life, as a preventive measure. He believed that such a practice had been advocated, but he did not feel inclined to support it.

Dr. Allchin, in reply, remarked that he had little to say because, after all, there appeared to be a very great agreement of opinion among the speakers. Mr. Sheild had accepted the idea of there being mild cases which did not need operation, but he had not defined these cases. He thought there was a tendency among surgeons to regard these conditions of appendicitis as necessarily, or almost necessarily, to be operated upon. This, however, must very largely depend upon the nature of the case. They must start upon the common ground that all these cases were of an infective character, and, in this respect, they were much indebted to Mr. Lockwood for the valuable contribution to the pathology of the disease with which he had opened the discussion. It was, of course, upon the actual circumstances of a given case that treatment must depend. Of the four classes, three called for operation sooner or later; how soon, he hesitated to say, not being a surgeon. When, however, it came to that very large proportion of cases which he claimed should not be operated upon, and which, he still ventured to say, must be regarded for all practical purposes as cured, they were apt to fall back upon some indication as to prognosis to make up their minds. Several speakers had referred to the rapidity of the pulse as the all-important guide in this respect, with which he fully agreed. He himself had never met with a case that had gone backwards and become severe, in which the pulse had not increased in rapidity. Under these circumstances one would take into consideration the other clinical characters in conjunction with the condition of the pulse, as Dr. Caley had pointed out in his paper and had emphasised the fact that the leading feature in the case was the actual rapidity of the pulse, and that it was only in exceptional cases that this would fail them. Mr. Sheild's incidental remark as to the absence of any definite swelling not being by any means to be regarded as a contra indication for operation, was also of value. Such cases as these were often more severe than those which were more obviously marked by physical signs. The speaker had already referred to the fluctuating character of the size of the swelling from day to day. Such cases, of course, might form, though he had no reason to suppose that they did, the majority of those which afterwards became recurrent. If that could be definitely shown to be the case, then, of course, it would be more desirable to operate upon them sooner. Lastly, with regard to the administration of opium, he had insisted upon this point because he felt that the statement in connection with the treatment of this disease in regard to the giving of opium, like certain other dogmatic statements as to the lapse of time before this or that was done, was that there had been a tendency to wholly dispense with the use of the drug and thereby neglect what he regarded as a valuable remedy. In urging that its administration was desirable and even necessary, he did not mean to say that the patient should at once be put upon an allowance of opium which would gradually bring him into a condition of narcosis. They must watch each case, and he maintained that they could far better estimate the severity of the symptoms under a moderate dose of opium, than when the patient was agitated and disturbed by pain. He had not heard of any positive objection to doing this, and he thought it was desirable that it should go

forth, as one of the issues of the discussion, that there was no very great objection on the part of surgeons in general to the moderate use of opium.

Mr. Lockwood, in reply, said he had tried to show that they could never come to an agreement as to treatment, until they could associate certain definite clinical symptoms with equally definite pathological changes. He had shown that appendicular colic was almost invariably associated with ulceration of the appendix, and he now asked them whether they would consider that, with such a disease, a patient was not in danger and unlikely to recover without an operation. He had hoped that someone would have discussed the question and told them that it was possible for such ulceration to heal and the patient become safe. With reference to Mr. Berry's remarks, he said there were cases for swabbing and cases for washing out. The sagacious surgeon did not bind himself to either method alone, but adapted the means to the circumstances. Each case must be taken on its merits. First, the diagnosis of appendicitis had to be determined, and, in his limited experience, that was not always an easy question to solve. A case on which he had recently operated had been diagnosed as a gall-stone by an eminent surgeon, as appendicitis by a physician—and the physician was right, and, as possibly, a stone in the kidney by himself. He had made an incision through which he was able to examine the three organs, and then only found out what the lesion was. The next step was to find out what kind of appendicitis it was. The proper course to pursue in all cases was not to minimise, but to remember that what is appendicitis with ulceration to-day may be appendicitis with gangrene to-morrow. He had waited to hear some speaker say what was a rapid pulse. Personally, he considered a pulse over 100 as rapid; he had frequently operated when it had reached 110, and had never regretted it. He commented upon the rather airy manner in which some speakers had alluded to abscess. One of the most difficult things to diagnose was appendicular abscess; in fact, he believed it could only be done in one way, namely, by looking in. Then the question of judgment came in as to what was the best moment to look in and see. In such cases probably two heads were better than one. Dr. Caley had not denied his proposition that some of his relapsing cases must have contained concretions. When Dr. Caley talked of a mild lesion, what did he mean? He would like to see it under the microscope, when it would probably turn out to be ulceration of the appendix with bacterial invasion. Such a lesion might become very grave after the patient had left the hospital. Indeed, Dr. Caley ought not to have spoken of a cure, but merely that the patient had left the hospital. The relapse was sometimes long deferred, and he knew one patient who had had a relapse after 15 years. Mr. Cripps had laid it down that after one attack of moderate severity he would not operate, but he himself would modify that rule, because he thought that after one attack a patient ought to be watched with great care, and, if there remained pain or discomfort in the region of the appendix, t became a matter for consideration whether the organ should be removed or not. No patient was safe if any trace of appendicitis remained. He would say that the appendix ought to be removed after one attack, if it had not got better after weeks or months. As to the question of operation after a second attack, all sorts of factors came in—age, social position, what the patient wished to do, whether he would remain within the

reach of surgical assistance if required, and so on. If he were going to the wilds of Scotland or to foreign parts, it might be unwise for him to do so. The question of when to operate was not half as simple as it appeared, and it was almost impossible to lay down dogmatic rules as to what should or should not be done. Personally, if thus afflicted, he would certainly send for a surgeon, and be treated on the lines he had laid down. In his choice of a surgeon he would select one he could rely upon to remove his appendix, and he would feel very unhappy unless he knew that it had been removed. Commenting upon Mr. Thomas's case of the young lady with the healthy appendix, he remarked that no one could say that an appendix was healthy until it had been examined microscopically. In conclusion, he believed that there were certain cases of appendicitis which were due to conditions of the lymphoid tissue and the lymphatics, and in which he had been unable to find bacteria.

March 5th, 19th, and April 2nd, 1900.

THE LETTSOMIAN LECTURES: PRACTICAL OBSERVATIONS ON CANCER OF THE BREAST.

By Sir William Banks, LL.D., F.R.C.S.

LECTURE I.

Experience and Statistics in Medicine.

When a man has to prepare an address the choice of a subject is generally a most difficult matter. If he has nothing of a very distinctly novel character to offer his hearers, it is probably best for him to give them his experiences on the subject with which he is most acquainted. If he has any judgment and discretion in him he may do something useful in this way, because in our profession advances are so rapid that they are apt to become bewildering. The record of a long personal experience under such circumstances may be of service as a kind of steadying medium. Moreover, these lectures, although nominally delivered to you, do really reach the majority of the medical public, and one of their prime uses is to present to that public at given periods a synoptical view of all that is known of the subjects they embrace up to the moment.

I will endeavour, then, to put into them my experiences of cancer of the breast during the past 33 years in which I have been engaged in surgical practice. It is true that, in the absence

of statistics, experience can only produce "impressions and recollections," and the professional statistician holds all "impressions and recollections" in the greatest contempt. He holds that figures can never lie. On the other hand, it has long been painfully manifest that nothing can be more lying and delusive than statistics if not properly used. In the first place, their collection must be made with the utmost accuracy, and with perfect purity of heart and calmness of mind. With medical statistics the temptation to gloss over and soften down unfavourable figures is very great, and requires that a man shall summon to his aid all his integrity. Again, when a man presents to you a vast mass of statistics, compiled at the expense of much time and labour, it is practically impossible to go over them with him, and you have no choice but to accept them. As a result, many errors have been long handed down from book to book, until some disagreeable doubter has arisen who has exposed them. Finally, even when the statistics are absolutely correct, the most erroneous deductions may be drawn from them, and from the same figures two men may extract the most opposite conclusions. If anybody wants to know how statistics may be manipulated and applied, let him consult the literature of the anti-contagious diseases, anti-vivisectionist, and anti-vaccinationist propaganda—all prepared by persons of the most superior virtue. Nobody wishes to deny to statistics their great value and their proper place, but one must also stand up for the value of those views which many successes and many failures have engrained on the mind of any man endued with an average amount of good sense and judgment. If, then, a man be not a waverer blown every way by the wind of opinion, he ought, at the end of an experience extending over the life of a generation, to have come to certain firm and definite conclusions about many things; and if he proposes making these views public, he must express them in a firm and definite fashion. This necessarily exposes him to the charge of egotism and dogmatism. The former I shall endeavour to avoid as far as in me lies, but should I occasionally become dogmatic, I pray you put it down to the exigencies of the situation, compelling me as they do to give my own particular views about cancer of the breast, while freely according to every other surgeon the liberty to advocate his views and declare mine entirely wrong.

The Increase of Cancerous Disease and its Cause.

Before coming to cancer as it specially manifests itself in the female breast, I should like to say something on certain points on the general question of malignant disease, which legitimately forms part of our present subject. Most medical men are of opinion that the number of persons who die from cancer in proportion to the number of the population has been steadily increasing. Very voluminous statistics from the Registrar-General's returns and from the returns of deaths in individual towns and countries seem to prove this very emphatically. My own "impression," apart from any knowledge gained from statistics, is strongly to the same effect. It is true that the "impression" of a surgeon who sees a great deal of cancer must be received with care, for it is doubtless the fact that, as a man's practice increases, he may see more of certain cases than he used to do, because his friends, knowing that he is interested in them, send them to him. Thus he may come to think that the same thing is happening in the practice of everybody, and that there is a general increase in those diseases all round, whereas the increase is in reality only in his own sphere of observation. In order to protect myself from falling into this mistake, I have summoned up my recollection of the year 1861–62, when I was a dresser in the wards of Mr. Syme in the Edinburgh Royal Infirmary. Mr. Syme then had a reputation as indisputably the first surgeon in Scotland: so that I am sure my old fellow-students of the period I mention will bear me out in saying that the pick of the surgical cases in the south of Scotland were sent to him. I have a very vivid recollection of the run of cases I saw, and possess a notebook to-day containing many of them. Now, cases of cancer, although not uncommon, were rare compared with the numbers in the practice of my colleagues and myself at the Liverpool Royal Infirmary at this moment. They were objects of curiosity and interest to us students. I can only remember once seeing a case of cancer of the rectum, so that I am able quite clearly, after an interval of 38 years, to assert that the number of cases in the wards of any one of the surgeons of the Liverpool Royal Infirmary at this day is much larger than I can remember in the wards of so distinguished a man as Mr. Syme, who drew his patients from a great area. This is more than a mere impres-

sion; this is a fact. The arguments used against the alleged increase of cancer are that it is more apparent than real, on account of the improved diagnosis of modern days producing greater accuracy in death certificates, and on account of necropsies being formerly the exception in hospital, whereas they are now the rule. In his very able article in the 'Practitioner' for April, 1899, Dr. Arthur Newsholme advocates these views with much ability. On these points I will with your permission, gentlemen, once more revert to my student experience. I think it may be fairly admitted that the character of the medical teaching at any given epoch is a barometer which gives the level of the knowledge of the mass of practitioners at that same period. In 1861, Miller, Syme, Spence, and Patrick Watson were the chief surgical teachers in Edinburgh. I assert that their knowledge of surgical cancer on the surface of the body and in the ends of the intestinal tract was as accurate as that of any surgeon of the present day. Bennett, Laycock, Begbie, Gairdner, and Sanders taught medicine, and I am unable to see any notable difference between their ideas of internal cancer and their methods of diagnosing it as compared with now. If mention be made of the brain, spinal cord, and nervous system generally, then, at the time I speak of, absolutely nothing was known about them, and the improvements in the diagnosis of them can only be appreciated by one who has witnessed the change; but this is not the case with the diagnosis of cancer.

I do not like to say much about the increase in the number of necropsies at hospitals, because I cannot make a positive statement on the question beyond this, that in my student days at Edinburgh (and I imagine the same holds good of all the great London hospitals) every effort was made to secure *post-mortem* examinations when possible. One would too gladly wish that Dr. Newsholme were right, but immediately following his paper is one by Professor Boswell Park (Buffalo), who states that in England and Wales the cancer death rate has risen from 1 out of 5,646 of population in 1840, to 1 out of every 1,306 of population in 1890; that is to say, that in 50 years the death rate has increased between four and five times, and he makes this astonishing statement:—"A careful study of all these tables permits one to make the following startling prophecy: If for the next 10 years the relative death rates are maintained, we shall find that

10 years from now, namely, in 1909, there will be more deaths in New York State from cancer than from consumption, small-pox, and typhoid fever combined." With every wish, then, to give a good deal to the theory of better diagnosis and more numerous necropsies, I cannot bring myself to believe that they account for more than a moderate amount in the increase of cancer mortality.

Local Distribution.

Has locality anything to do with cancer? This is a matter upon which a great deal hinges, but from personal experience I cannot give any information. It is unlikely that any hospital surgeon will be able to do so, for he who has obtained any reputation for the operative treatment of cancer draws his patients from such varied sources that the question of locality as affecting them is void. On this subject information of any moment can only be obtained by collecting great masses of statistics, and even then the deductions therefrom will have to be made very cautiously and judiciously. The first attempt in this direction was made by Mr. Moore, of the Middlesex Hospital, in 1865, and to him is also due the merit of being the first who boldly drew the attention of the profession to the inefficient operations for cancer which were performed in his day. To his courageous protest is owing the modern principle of free removal. But, as we all know, Mr. Haviland's labours have been those of a lifetime, and his tables of statistics are a marvel. In 1868, to the Medical Society, in this very room, he read his first paper, "On the Geographical Distribution of Cancer among Females throughout England and Wales during the Decennial Period 1851–60." He found that 4·33 cases of cancer occurred in every 10,000 females, and 1·94 in every 10,000 males. Leaving untouched the decennial periods 1861–70 and 1871-80, he has investigated 1881–90. He then found 7·30 cases in 10,000 females, and 4·30 in 10,000 males—that is, 0·42 more than double among males and 1·28 less than double among females. Admitting these statistics as true, two things are clear: (1) That there has been a great general increase of cancer all round; (2) that this increase is much more marked in males than in females. The following are Mr. Haviland's "last words":—

"1. That the districts having the highest death rates from cancer among females were invariably associated with seasonally-

flooded areas traversed by, or in close propinquity to, seasonally-flooded rivers.

"2. That geologically these high mortality districts were characterised by alluvium and subsoils of clays of every variety of age and formation.

"3. That the districts having the lowest death rates from cancer were situated on elevated land, where the drainage was good and the physical features of the country such as to preclude the possibility of floods; where rivers derived their sources, and where, in fact, they were not fully formed.

"4. That geologically these low mortality districts were characterised by the oldest palæozoic rocks, especially those of the carboniferous limestone period—the liassic, oolitic, and cretaceous limestones.

"Thus at the end of this brief sketch of the medical geography of cancer we find ourselves asking the same question that rose to the lips of my audience in November, 1868: How is it that limestones are always associated in England and Wales with the lowest mortality from cancer, and flooded clays with the highest? This question still remains unanswered, and will still so remain until the subject is taken up in earnest by the medical profession, for it is a field, I find after 30 years of working in it, that cannot be made to produce fruit without the toil of many labourers. When the above question is answered light will begin to fall on the cause of cancer."

It is a most refreshing thing to find this veteran statistician placing his figures at the disposal of his fellow practitioners, modestly admitting his inability to draw sure and certain conclusions from them, and encouraging other workers to supplement his labours, so that some determination on a most important question may be reached.

Let us turn to Mr. W. Roger Williams's most interesting work on 'Diseases of the Breast,' published in 1894, a work evincing an infinity of statistical learning. It is a model of how research by statistics should be employed, in which figures are fairly handled and deductions prudently made. Furthermore, it is a mine for all those to dig in who are interested in the question of cancer. Mr. Williams thinks there is much to be said in favour of the opinion that in this country cancer is specially prevalent in

flat, low-lying, fenny districts, noting especially Cambridgeshire and the adjacent counties of Northamptonshire, Huntingdonshire, and Bedfordshire. He observes, however (taking his figures from the Registrar-General's forty-seventh report), that all low-lying and seasonally flooded districts have not a high cancer mortality. The very large area drained by the Severn and its tributaries, for instance, has throughout a low average mortality from this disease. In fact, as we shall see presently, his view of the cause of the increased cancer mortality is very different and is not geological at all.

One would very much like to enter into the question of cancer houses, but it is, perhaps, digressing too far from my subject, which really has relation to the disease as it occurs in the breast. Mr. Haviland, Mr. D'Arcy Power, and Mr. Law Webb, of Ironbridge, in this country, as well as certain French and German surgeons, have written papers on the subject of profound, indeed almost terrible, interest. Still more evidence, however, will have to be forthcoming to make a clear case; for the liability to fallacious deduction in such a subject is so serious that a great mass and weight of testimony in its favour will yet have to be produced.

Cause of the Increased Cancer Death Rate.

Up to the present time, while statistics have already cleared and are at this moment rapidly clearing up many points about cancer, nothing definite has as yet been furnished by them as to the reason of its increase. That being so, there is a fair and reasonable ground for the employment of the "impressions" begotten of experience. For many years, the more I have thought over this matter the more forcibly does the idea present itself to me that it may be due to the great increase in the amount of food consumed by the people of this country during the past half century. At all events, this view is worthy of being argued. I think it was first put into my head a long time ago by Sir James Paget. I took to him a lady with incurable cancer. He gave her some directions, and one was that she was to eat as little as possible; to try and starve herself, in fact. His long experience had taught him that to starve the body as a whole probably meant starving the cancer locally. *Per contra*, if that be a correct notion, then cancer ought to be connected with well-nourished bodies.

Certain it is that the disease has not appeared to me to affect the weakling, the tea-shrivelled, mummy-like old maid, or the ill-fed, more than half-starved drunkard who never has any money to spend on good food. Its most numerous victims are well-nourished persons, with plenty of beef and fat about them, and often with a fine, healthy colour in their cheeks. In summing up the many operations I have done for cancer of the tongue, my recollection of the patients is for the most part of biggish, powerful men, with large, strong jaws which were hard to saw through when sawing had to be done. The better the nutrition and the younger the patient, the more deadly and rapidly growing is the cancer. Take what is termed a fine "buxom" woman of 30 or 35, with cancer of the breast, and her chance of permanent cure, even by the most extensive of operations, is almost *nil*. It is true that cancer is uncommon in the young during the period of development of the body and of the functional activity of organs, so that in one sense it is a disease of "degeneration"; but this has nothing to do with the good nourishment and muscular development of the body in middle life.

Being assured in one's own mind that well-nourished, well-developed, healthy people are the most numerous victims of cancer, the question at once arises, Is the increase of cancer in any way synchronous with an increase of nutrient material—that is, of food—throughout the country, and are people, in consequence, better nourished? There cannot be a doubt of it. Ever since the passing of the Corn Laws bread has been cheap and plentiful, while during the last 20 years the importation of animal food from other countries has been enormous. The increased wages and emoluments of all classes in this country have enabled them to purchase freely of the best there is to be had in the whole world of things to eat and drink. Our working classes fare admirably. Our better classes eat infinitely too much, especially of animal food partaken of at breakfast, lunch, and dinner. But for the athletic tendency of the age and the general passion for games and exercises which pervades all classes, this over-stuffing must have proved very dangerous. I am pretty well convinced that, when a man is over 45, excess in food is perhaps worse for him than excess in drink. Apparently one of the results of too much nourishing food is the production of a widely-spread, second-rate kind of gout of a different type from the

acute and furious attacks produced in former days by the copious drinking of beer and port wine. Moreover, I think distinctly that it has also to do with the production of the cancerous diathesis. This is also the opinion of Mr. Roger Williams, who, while admitting Mr. Haviland's topographical statistics, cannot agree with him in his geological views, but inclines to think that cancer areas are also the areas where the people are best nourished.

The stock answer to what may be called the over-nutrition theory is that vegetarians and rice eaters are just as subject to cancer as gross feeders and flesh eaters, and this statement goes on being very steadily repeated from one book to another. I am not prepared to admit this. Cancer is not so rife in Ireland, where feeding has been poor and very largely of a vegetable character, and where improvement in the living of the people has not proceeded *pari passu* with that in England. The Registrar-General's report for the decade 1881–90 shows that in 1881 in England 1 death in 36 was due to cancer, and in Ireland 1 in 47, while in 1890, in England there was 1 in 28, and in Ireland 1 in 40. Ireland, therefore, does not rise to the level of England. Mr. Roger Williams has a most interesting chapter of facts in this connection. He tells that Billings of the United States showed that there were 2 cases among the whites to 1 among the blacks. From what can be learnt of the whole continent of Africa, that vast country enjoys a remarkable immunity from the disease, especially the Egyptian area and the northern parts. At the Cape cancer is common among the whites, but very rare among the natives. All through the tropics the disease is distinctly rare. In India the general opinion is that it is not common, and that, when it does occur, neither the rice eaters nor the meat eaters are exempt. It is true that certain surgeons (McLeod, Hendley, and Lawrie notably) hold the opposite opinion, but I notice that their experiences are drawn from hospital work almost entirely, and refer chiefly to the number of operations occurring in large hospitals. This seems hardly a sound argument. All that it proves is that a number of cases of cancer come into large hospitals, and consequently that in a huge country there must be many cases of cancer. But if statistics are to be of any avail, the proportion of cases to the actual population is absolutely necessary to make any fair

deduction. It is like the death rate from snakebite. To an Englishman who never sees such a thing, the sum total of these seems appalling. It is not till one takes the proportion to the astounding population of India that the insignificance of the loss of life becomes apparent. If there were no knowledge of the exact population of Lancashire, and if a person were to walk round the wards of the Liverpool Royal Infirmary and estimate the cancer mortality of the county from the number of cases found in these wards, his view of the prevalence of the disease would be a most alarming one.

At this moment the Massachusetts State Board of Health is instituting an inquiry into this question. Its circular states that—

"Every year there is an increase in the reports to the State of the number of deaths from cancer, even when allowance is made for age and greater population. By some it is alleged that this is due to an increase of the disease itself, by others that better diagnosis and more careful registration will account for it. Furthermore, the infectious nature of this disease, caused by a possible parasite, and its contagiousness, have been strongly urged."

I believe that the great general increase of cancer is for the most part admitted, and that Dr. Newsholme's idea that it is not a real, but only an apparent, increase, due to better knowledge and more accurate registration of the disease, is entirely erroneous; but even were we to admit his views, it would not account for the fact that during the past 30 years the comparative cancer mortality of the sexes has altered in a most singular manner, so that, whereas the disease was formerly more prevalent among females, it has now become more prevalent among males, and is becoming increasingly so.

Now do drunken habits account for more cancer? I am certain that confirmed drunkards are not more liable to it than other people; besides, drunken habits are rather diminishing than increasing. Does syphilis account for it? I have never been able to trace any connection between that disease and cancer whatever. It is true that leukoplakial bald patches and specific surface ulcers of the tongue and mouth are places where epithelial cancers arise, but they do so, not in virtue of any special syphilitic

taint, but just as they arise from irritation of any kind, as from a sharp-edged tooth. Mr. Williams, out of 165 female breast cancer patients, did not find a single one of immoral character. Do bad water, bad housing, and bad sanitary arrangements account for it? This is impossible, for in nothing have we improved so much as in improved sanitation, in improved water supply, drainage, housing, and clothing. Will increased nervous tension from our rapid existence, due to the development of steam and electricity, account for it? It would seem reasonable to suppose this likely to produce all sorts of nervous trouble and arterial or cardiac degenerations, but not cancer. Is it due to the fact that there is a great saving in the early periods of life, and that more people live into what may be called the cancerous age? The Registrar-General's statistics show that the number of persons of 25 years of age and upwards, out of every million living persons, was just the same in the decade 1881–90 as in the decade 1871–80. There has, therefore, been no increase in the proportion of persons who live into the cancerous age.

What great change, then, has taken place in the national life? Richer and more abundant food. You will note particularly that it is the male who eats the heavy food in ever-increasing quantities; while the female remains much as before in her eating, and note along with this that it is men now who suffer most severely from cancer and not women, as formerly. Again, in all England the most tainted places are London, the Thames Valley, and the counties adjoining the metropolis, where the most luxurious habits and the best living prevail, and it has been pointed out that even in the capital itself, the parts where those in easy, comfortable circumstances live, who are supplied with more than the necessaries of life, show the highest mortality. It is curious to note that amongst the very highest mortality rates for occupations are those which include commercial travellers, coachmen and grooms, merchant seamen, maltsters, brewers, innkeepers, butchers, and plumbers. Most of these certainly are persons likely to eat and drink abundantly, and not take too much exercise; persons who live well and do not work off their spare products. Singularly enough doctors figure low in the list. In conclusion, let me quote Mr. Moore's statement, made many years ago, that "cancer is eminently

a disease of persons whose previous life has been healthy, and whose nutritive vigour gives them otherwise a prospect of long life."

The Paramount Importance of Early Diagnosis.

As one goes on seeing case after case with gentlemen in general practice, one gets to find out that, even with the highest standard of modern education, a great many errors are still being committed by doctors in the matter of the early diagnosis of mammary cancer. Now, seeing that in the matter of its cure we have nothing but free removal to fall back upon, the smaller the amount of disease the better the chance of cure. Early diagnosis is, therefore, of paramount importance. If this can only be steadily crammed into the minds of students and practitioners, a remarkable increase in the number of permanently successful operations will ere long result. This is the real line of improvement at the present moment: not in devising extravagant operations for cases that are past praying for. It is true that in the very early stages of a mammary cancer a certain and positive diagnosis cannot be made, even by the most skilled and experienced surgeon. Nobody, however, expects that of the general practitioner. But I have found that when the patient comes and shows her doctor a small lump in the breast, if it does not seem anything very formidable he is unwilling to frighten her, talks vaguely about a "swollen gland," and generally is apt rather to make light of the matter. There is a phrase I know well which must have cost a great many people their lives: "If it doesn't bother you, don't bother it." It is a most destructive axiom. Then the patient goes home, the lump grows and she sees it growing, but she buries her head in the sand, and repeats to herself that the doctor did not attach any importance to it and so nothing can be going wrong. In due time she becomes terrified and goes to the doctor. There is a distressing scene, and then they repair to the surgeon, who says he thinks he can get the disease out, but he is not sure.

Now, what I would implore my brethren in general practice to do is to cease to regard any lump in the breast, however small, as harmless or unworthy of the utmost attention. No man can tell what is going to become of a small lump in the breast. All strangulated herniæ are now operated upon without delay if they

will not go back; so all lumps in the breast ought to be cut out if they will not speedily disappear. As to frightening the patient, there is no necessity for alarming her; but, even if it were so, how much better that she should get a needless fright in the beginning than be ultimately terrified by the dreadful intimation that her doom is sealed, and that a brief period of agony is all of life that is left to her! My advice to a general practitioner would be that, when a patient comes to him with the smallest lump in the breast, he should at once send her to a surgeon. If she be a poor person, he will surely have a surgical friend who will see her gratuitously for him. If an absolutely certain diagnosis cannot be made on the moment, the point is to get the patient to visit the doctor month by month to let him see what is happening to the lump. It seems hardly credible, but on several occasions I have seen a patient in consultation with her own medical man about an early mammary swelling. It has been impossible just then to give a certain opinion about it; treatment has been suggested, and a strong recommendation made that the swelling should be carefully watched. And yet, after a year or more has elapsed, they have so muddled up matters between them that the patient has been brought back with a pronounced cancer. This is a statement of fact and not an ideal picture. We have just been considering various conditions relating to cancer, upon which have been brought to bear ingenious speculations, supported by statistical researches involving years and years of labour. All these researches are not of the faintest consequence (considering the nature of the solitary method of treating cancer of the breast which we possess), compared with efficient early recognition of the disease. Every effort should be made by teachers to inculcate the doctrine of early recognition upon students for, if there is a hopeful day to come for the sufferers from cancer of the breast, it will be when women have been taught by the medical profession to present themselves on the slightest symptom of anything being found wrong with the mammary gland. Too often at present they hug their distressing secret, and only part with it when it cannot be concealed; and small wonder it is, when one reflects that the present generation are still under the influence of the distressing tales told them by their mothers about the horrors of cancer of the breast, and the terrible and useless operations performed for its relief. I hope my medical brethren

will forgive my speaking in this very forcible manner about what appears to be a very simple thing. It is a very simple thing, but, simple as it is, it is the real secret of the future of the operative surgery of cancer, and we are not getting what we ought out of it.

Let me not be supposed to reproach my profession with ignorance (for I trust I can recognise the many mistakes I have made myself), but to expect omniscience from the general practitioner is absurd. I consider that the range and variety of his knowledge are astonishing, but the early diagnosis of many mammary enlargements will defy the most experienced surgeon. It is only by the early co-operation of the two that the patient's chances can be saved to her. Were I a younger man I would not dare to say this.

Constitution and Heredity.

One of the terrible mistakes of former days, which gave cancer the character of being even more hopeless than it is, was the idea that it was purely a constitutional disease. To remove it in one place was useless, for it would just break out in another. Now the modern scientific inquirer, whose first consideration is too often to sweep contemptuously aside all that his predecessors have excogitated, asserts that there is nothing constitutional whatever about cancer; that it is a purely local disease, and that it is certainly an infective one. It seems odd why there should be any difficulty about the question. It is a long time since Laycock first described the diathetic tendencies of the body, and how they influenced all the illnesses which attacked that body on its way through life and often so modified them that they were almost unrecognisable, so changed were they from their usual types. Paget used to draw our attention very vividly to the remarkable way in which the gouty diathesis affected surgical disease. We know, nevertheless, that it does not necessarily follow that, because a man has a diathetic tendency to a disease, it need ever become obviously manifest. He may go through a long life and never show it until some *contretemps* brings it to the surface. An old friend of mine, whose male relatives to a man were all full of gout, lived to over 70 without a trace of it. He drank champagne and port most days, but he was small and thin and a passionate sportsman, so that he staved off the gout by great physical

exercise. But at last one day he fell off his shooting pony and hurt his ribs. While lying in bed recovering from the injury he had a serious attack of gout in the foot. For the next two or three years of his life it never left him, and it killed him with acute cerebral trouble at the last. If in place of "gout" we were to put "cancer" into this little case there would be really no difference. But that cancer should in some way be set agoing in a particular spot of a perfectly healthy man's body by the process of infection, and should then produce a constitutional state apart from mere spreading by lymphatic channels, is with our present knowledge difficult to realise. If you were to take a fine, wholesome, clean-blooded man, the descendant of a race who had for generations been free from all morbid diathesis and put tubercle into him, you could not make it spread. It would die out locally. It requires a distempered constitution, like a sour impoverished soil, to give the appropriate pabulum for pathological weeds, which seed and spread through it and ruin it. We know now how the tubercle weed gets sown. The cancer weed may yet be caught and bottled too. When we think what an absolute revolution has taken place in our views about tuberculous disease, we must not despair about cancer. It is a curious thing, however, to remember that, while the North Italian peasants regarded phthisis with the greatest dread as an infectious disease, scientific medical men were the very persons who scouted the idea. Long-continued experiences are worth something after all.

As to heredity, it behoves us to be very careful how much importance we are to give to this in a difficult case of diagnosis of a breast tumour, where side issues have to be taken into account. By the public without doubt too much value has been always attached to it. The patient consoles herself with the belief that hers cannot be a cancerous tumour because nobody in the family was ever known to have such a thing. Unfortunately we can only put the matter in this way: While the want of history of a cancerous tendency inherited from her progenitors is no proof that the patient's disease is not cancer, the existence of it among several near relations is distinct evidence in the direction that it is so. There have been attempts made on certain abstruse developmental grounds to overthrow the possibility of the hereditability of acquired (diseased) variations, notably by the philosophic teaching of Weissman, but nothing has come of them.

A great deal of philosophic froth has been beaten up and much learned verbiage has been poured about, but I have never seen abstract philosophy do much in disease. Our knowledge—our most useful knowledge—has been gained empirically; and so patient collectors of facts have told appalling histories of whole families who have died from cancer, and have disseminated it among their descendants, which are now so numerous and well authenticated as to leave no doubt in the mind of any surgeon (not a philosopher) as to the existence of heredity, and what we have to do is to attach to it its appropriate value clinically, and by no means to consider that the absence of heredity is proof of non-malignancy in a tumour. It is true that, as a general rule, cancer in a family seems very erratic in its visitations, and appears to jump over a generation, but we may take a simple view of the question thus as explaining these irregularities. What figures seem to show is that the inherited infectivity of cancer is not strong, and dies out most likely at the end of a couple of generations. In the midst of a family among whom cancer was never known, a healthy man develops (catches?) cancer, and throws the gloomy pall of hereditary diathesis over his children and grandchildren. A son exhibits cancer, but his children escape, while his niece—the originator's grandchild—exhibits it. The rest escape because the match has not been applied to the tinder, just like my friend who escaped hereditary gout till he met with an accident towards the end of a long life which set it ablaze in him. It is impossible to resist coming again and again to ask, What is the match? Meantime, until a very clear answer is given to this, I see no occasion for throwing overboard what knowledge we already possess.

Dr. Boswell Park, in his chapter on the frequency and nature of cancer, in the 'Practitioner,' already alluded to, writes as follows:—

"Next, with regard to the actual nature of this disease. First of all, it is possible to sweep aside, with one or two possible exceptions, everything that has been brought forward until recent years as an explanation of this disease. The old theories of irritation, of perverted nerve centres, of heredity, &c., cannot for a moment stand in view of the searching scientific methods of to-day. We have gotten as far as this at present, that we know

that cells act as they do in producing the disease either as the result of some external or of some internal stimulus. The exact character of this stimulus is not yet certainly demonstrated, though things point more and more towards its parasitic—that is, its potent living—influence."

To the first part of this statement I entirely demur, because I am so old-fashioned as not to have got far enough to sweep aside all that experience has accumulated until I have something to put in its place. With regard to the second part, it seems to involve on the part of Dr. Park what an old Scotch judge called "a plain perception of the obvious." What we all want to know is, What is the match? and there is no certain answer to that as yet.

Traumatism.

We are all very prone to produce some distinct reason why a disease should have come to us. That it should come spontaneously is a stigma upon the soundness of the constitution, and so we wish to find out a cause. But the natural liveliness of the female imagination makes it very difficult to apportion with anything like accuracy the exact amount of credence to be placed in their statements. I must say that I have never been able to make out that milk abscesses or the scars of incision for them have ever been the starting points of cancer; and, curiously enough, although the patients have mentioned the existence of these things, I do not remember having heard them distinctly and directly attribute their growths to them. After all, if cancer ever develop in inflammatory products, it is in those of a very chronic and not in those of an acute nature. I never heard, furthermore, any statements from the patients that pressure from corsets or that irritation of the nipple by suckling had produced their complaints. On the other hand I have very often heard most clear and circumstantial histories narrated in which blows were asserted to have been the primal cause of the trouble. It has been held as an argument against the effect of traumatism that men are very much more liable to blows on the breast than women, and most rarely have breast cancer. I very much doubt this. Take a number of men at 50, and an equal number of matronly, full-bosomed women of the same age, and it stands to

reason that the latter will get more knocks on the mammæ than the men. Moreover, they are always in the way of getting knocks from young children or from articles of furniture, such as cribs and bedposts, among which they are always working. Besides, what is a man's breast after all? A little mass of fibrous tissue with some rudimentary gland stuff in it, whose small size would save it from many blows which a big, soft, vascular female breast must encounter. Moreover, it is contended that if blows caused mammary cancer, the lesions would be multiple, but this just shows how things appear differently to different people, for I do not suppose that if a woman gets an injury to her breast, she gets it in the form of a series of thumps all round, so as to produce multiple cancers. I have received very succinct accounts from intelligent women of blows from pieces of furniture or kicks from young children, in which there was pain immediately afterwards sufficiently severe to fix the exact place in the breast in the memory. Then the pain has gone away, and they have almost forgotten the circumstance, until some day a lump is found at the old site of injury. Were the allegation true that the injury directs the woman's attention to a tumour which is lying concealed in her breast already, then she would discover it at once on receipt of the injury, but this is not the history of these cases. The history is that of an interval, after which the lump comes painlessly. Many of the patients whose histories I have ascertained were far too intelligent to have invented the story after finding the lump.

Mr. Roger Williams says that in examining the cases of 137 women, one out of four made statements as to previous traumata. I should hardly have thought so many would have done so, for supposing we put one-half of them out of count at once as imaginary, it would leave one case in eight in which there was not improbably some truth in the patient's statements. After the way in which the vulgar but rooted belief in the infectiousness of phthisis of the Italian peasant has, in the light of modern discovery, triumphed over the contemptuous disbelief of the whole world of medicine, I am not disposed to abandon all belief in the statements of women, whom we will even admit to be anxious to find a cause for their malady. Constant repetition from different sources strengthens even a weak tale, provided the evidence be steadily in the same direction. I have investigated these stories

too often not to feel that, stripped of all exaggeration, there is a distinct element of truth in them. Remember I do not make any attempt to assert that cancers of traumatic origin are at all frequent, but I believe in the possibility and, indeed, in the strong probability of their occurrence. I cannot see anything irreconcilable with our pathological knowledge in the idea of a blow leaving behind it a certain limited area of chronic inflammation in which the gland epithelium should begin to grow and then burst its bounds, just as I am certain it does in chronic mastitis. Dr. Boswell Park says:—"The supposed intimate relation which injury bears to cancer is of no more importance than that upon which the clinicians and even the pathologists used to place so much stress in tuberculosis. There are still those who claim that the predisposing cause of most tuberculous joint diseases is previous injury of some character, and there are still those who look for a similar mechanical cause of cancer in virtually every case." I am not one of those who look for a mechanical cause of cancer in virtually every case, but I venture to say that there is no practising surgeon in this country who has much to do with joint disease who does not recognise the immense importance of wrenches, blows, and sprains in the production of the early inflammatory changes in and about joints, which ultimately end in what is known as articular tuberculous disease. I was educated in exactly the opposite view—namely, that the tales which mothers told about injuries to their children's joints were all nonsense, and I was forced out of this belief by the irresistible arguments of experience.

One feels the more inclined to this view from the knowledge that acute traumatic malignancy is a well-recognised disease. Scattered throughout the medical journals are so many cases of its occurrence that no possible doubt can be entertained about its existence. In the 'British Medical Journal' for 1882 are two short but very interesting papers on the subject by Messrs. Barwell and Harrison Cripps, also a third by my friend Mr. Puzey, of Liverpool, detailing in a most interesting manner the history of a man who was struck over the sternum, and in whom there was developed at the spot a sarcoma. It is true that in all the cases about which I have read, sarcomata and not carcinomata supervened, and therefore the disease is rightly named "acute." But if injury will, so to speak, on the spot

start into existence the swift-growing tumour of the connective tissue, why should it not at a longer interval give rise to the slow-growing tumour of glandular lining epithelium?

LECTURE II.

Some Popular Errors in Diagnosis.

There are some delusions about cancer of the breast to which almost the entire public still give credit, and, I regret to say, not a few medical men. They are partly the result of textbook statements not properly explained and partly the result of popular tradition. They are difficult to eradicate, because there is just that small amount of truth in them which makes it possible to argue about them. But they are very deadly and dangerous delusions, because they cause loss of time, and, as we have just been seeing, that is destructive of the only chance of complete cure—namely, early operation. I would reckon among them errors about heredity, about pain, about cachexia, and about nipple retraction.

Error about Heredity.—This question we have already discussed and need not further refer to, except to repeat that the absence of any cancerous taint among relatives is to a woman a very strong inducement to say nothing about any suspicious growth she may feel in her breast, because she comforts herself with the idea that it cannot be cancer, as such a thing was never known in her family.

Error about Pain.—This error is a most serious one. A few internal cancers—such as those of the liver and lungs—are sometimes almost painless in their whole course; but the termination of all external cancers, at all events, is invariably accompanied by severe suffering. These dreadful cases appeal so distressingly to the minds of women that they always associate pain and cancer. But unfortunately there are no neoplasms which produce less pain in their early stages than cancer. As the female mind, then, invariably associates the presence of pain with cancer, so its absence means to the patient that there is no cancer. If she has no pain in the lump and never heard of the disease among

her relatives, she is very unwilling to tell the doctor what she has in her breast. This remarkable absence of early pain is regarded as all the more of good omen by reason of the fact that, in a large proportion of cases, the patient discovers the lump in her breast by accident. Men do not, as a rule, wash their breasts very much, but women do, and certainly the most frequent way in which the patient tells the story of the discovery of her enemy is that it occurred as she passed her hand over her breast while washing. So far from regarding pain as a sign of cancer, when a patient comes to tell me that she has detected something abnormal in her breast and that it is very painful, I am always glad to express my pleasure at hearing this statement, inasmuch as the disease is then probably not malignant. Pain in certain cases is by no means an evil.

Error about Cachexia.—The error about cachexia is chiefly made by doctors. Often, after examining a patient with some medical friend, I have been struck by his remarking, "Well, but look at her ruddy colour and healthy aspect. There is no cancerous cachexia there!" Like pain, we have here an old superstition, a deeply rooted, popular belief to the effect that a patient who has cancer must have a pallid, emaciated, haggard appearance. When fœtid discharge and agonising pain, and sleepless nights and mental distress, and absorption of septic products have done their work, then it is true that the unfortunate patient does present the appearance of a cachexia, but certainly not at the beginning of her disease. We have already noted that the victims of cancer are usually well-nourished persons, often with a fine healthy colour in their cheeks. Many a time I have pointed out to students in the out-patient room some jolly-looking, ruddy-faced man, with hair just turning grey, a big comforter up to his nose, and a tuft of cotton wadding sticking out of one ear, and have said, "That man has cancer of the tongue." In cancer of the external parts cachexia is practically of no aid diagnostically, because in its early stages there is none, while in the later stages the growth is apparent. But I admit that in certain forms of intestinal and other internal cancers, where the disease is long latent, the fact that the patient has lost his ruddy colour and become pale and waxy, that his aspect is pinched, and that he has lost flesh and shrunk in his clothes—in short, the existence of cachexia—is an important point.

Nipple Retraction and Skin Adhesion.—Any error with regard to these can only be made by the doctor, and not by the patient. Perhaps the majority of textbooks have hitherto somewhat misled the student on these points. It has been given out too broadly that retraction of the nipple is a sign of carcinoma, while the statement has not been sufficiently qualified by clearly indicating to the tyro that the sign is only of the least value when the disease happens to be situated immediately beneath the nipple. If the tumour has its site at any distance from the central point then retraction naturally does not occur. To persons thoroughly conversant with the matter, it seems strange that it should be necessary to point out to anyone that the central point of the mamma can only be interfered with by disease that underlies that central point. As an actual fact, however, I know that the mere presence or absence of nipple retraction, without reference at all to the position of the cancerous mass, is too often appealed to by young practitioners. But if the absence of nipple retraction is no diagnostic sign in cases where the disease is situated away from the centre of the gland, on the other hand, it is practically pathognomonic when it lies beneath it.

Adhesion of the Skin, as we all know, is a very grave indication of a sublying cancer. It does not occur with any other tumour except an inflammatory swelling of the nature of an abscess. But there is a different appearance between the skin in the two cases. In the inflammatory adhesion the skin is fairly smooth over the abscess, or whatever of an inflammatory nature underlies it. But over a carcinoma, if you try to pinch the skin up between the finger and thumb, immediately a number of fine points appear, which are the hair follicles caught and tucked down by the cancer. It is exactly like the leather of a saddle, insomuch that I have always demonstrated it to students under the name of "pigskin." I regard this "pigskin" appearance as almost certainly indicative of subjacent carcinoma.

What are the Conditions which most Closely Simulate Mammary Cancer?

Chronic Mastitis.—To recount one's own mistakes is the most telling way of communicating information to others. I know that I have several times confounded a piece of chronic inflammatory

induration with carcinoma. I can recall very clearly three cases. Curiously enough the three patients were all married women between 35 and 40 years of age, comparatively spare in form, and childless—a kind of person, by the way, who has always seemed to me more prone to chronic mammary induration than the stout plethoric woman who has had a big family. When the condition becomes pronounced, the whole breast comes to resemble a hard flat cake. You can lift it off the pectoral muscle in a mass. Here and there throughout it are points of extra hardness, like knobs or bosses. One of these, bigger than its fellows, is the suspicious object. The question comes to be, Is that hard knob simply a bit of extra-hard chronically indurated tissue, or is it a piece of malignant disease? In point of mere hardness there is nothing to choose between either condition. Feeling alone gives very little help, while the difficulty is increased by the fact that throughout the rest of the breast are points which are only a little less in size and a shade less hard than the one which has given rise to suspicion. It is very difficult to give any precise rules by which to recognise a bit of indurated mastitis from a bit of carcinoma. Experience alone helps. But it may be noticed that in mastitis skin adhesion is very rare, and so is glandular enlargement. The adhesion, if it exists, is not of the "pigskin" type; while, when axillary glands are to be detected, as they are in thin women, they are so hard in cancer that this feature alone distinguishes them.

Small Chronic Abscess.—Probably the thing which is next most commonly mistaken for cancer is a small, deep collection of pus, surrounded by a zone of inflammatory thickening. Not many years ago I saw a surgeon whip out a breast for a supposed scirrhus. When the breast was cut up in the lavatory, before being handed round to the students, the tumour was found to be a small abscess with a condensed mass of inflamed tissue around it. At the first blush one would think this mistake a very unlikely one to happen, but it is not so. The deep-seated, ill-defined, hard, inflammatory mass, surrounding so small a quantity of pus that fluctuation cannot be detected in it, has a marvellous resemblance to a piece of carcinomatous tissue. Moreover, the delusion is heightened by the fact that there is often a little adhesion between the skin and the swelling beneath, but, as we have just seen, it is not of the genuine "pigskin" order.

A Small Fibro-adenoma.—A condition very like the hard point in a case of chronic mastitis, to which allusion has just been made, is produced by a small fibro-adenoma. I recollect a very nervous woman coming into hospital with what I believed to be a tumour of this kind. She was extremely timid, and only after considerable pressure consented to have the growth out, and that on the express understanding that nothing more was to be done; but when we came to the growth it was found to be scirrhus, although the signs of a fibro-adenoma had been so marked that I had been at some pains to point them out to the students as very characteristic of that form of tumour. I removed it freely. The patient was told what had been found, and was begged to return and report herself from time to time. This, however, she never did, being a most foolish and nervous creature, and we saw nothing of her for a considerable period, until she came back again with the whole breast a hopeless mass of cancer. In point of hardness a small fibro-adenoma scarcely differs from a scirrhus, but the great diagnostic point is the absence of the "pigskin" and the total want of all glandular enlargement.

A Small, very Tight, Simple Cyst.—I learnt to correct this first from seeing a surgeon of great experience proceed to cut out a breast for cancer. He gave his reasons for thinking that the tumour was malignant, but said that, as the growth was only small, he would cut into it before removing the breast, in order to make assurance doubly sure. He did so. A tablespoonful of brownish fluid escaped, and the tumour disappeared. I once afterwards did the same thing before a crowd of students, even though perfectly aware of the liability of mistaking a very tense cyst for a scirrhous mass. Many years elapsed, and I did not fall into the trap again until a few months ago, when I diagnosed a very deep-seated, tight cyst as a malignant growth. If the cyst is near the surface of the mamma the difficulty is not great, but if it is deep down its margins become obscured. The diagnostic features are that the tumour is round, smooth, sharply-defined—unless it is very deep—and freely movable, while there is never any skin adhesion or glandular enlargement by any chance.

I was very pleased to notice that Mr. Marmaduke Sheild, in his excellent work on 'Cancer of the Breast,' says with regard to the diagnosis of carcinoma from cysts and abscesses:—"It is in these cases that the most disastrous errors are made in matters of

diagnosis of mammary disease. It will be well at the outset to point out that if any surgeon thinks he is clever enough to certainly distinguish between solid tumours and some of the more peculiar abscesses or cysts found in the breast without exploratory incision, he will sooner or later make an egregious error."

In connection with the diagnosis of mammary tumours there are one or two points of practice which, if employed as a matter of routine, might save an operator from occasionally falling into an error of what may be termed a public character :—

1. *I never remove a breast without first cutting into the tumour* so soon as the patient is anæsthetised, and on sundry occasions I have been extremely glad that I did so. I acknowledge to constant mistakes, being aware that omniscience is the exclusive property of very young men.

2. *I frequently use a small exploring trocar and cannula for diagnosis.* I know that this practice has been much condemned. I was taught that it was very dangerous, but I found by practical experiment that it was nothing of the sort. It was held that it inflamed the tumour and accelerated its growth, and so, if the tumour were inoperable, it did damage to the patient. This view was quite true, because surgeons formerly put large and dirty trocars into the interior of solid tumours, and inoculated them with septic material. If an exploring trocar and cannula with a metal handle be used, which can be boiled and then passed through a spirit tube, no harm whatever will result. On the contrary, many dubious questions can often be settled at the cost of a most trifling amount of suffering, and to the enormous mental relief of the patient.

3. *I always examine a breast tumour in three positions.* In examining, say, the left breast in the first position, the surgeon should stand behind the patient, with his left arm passed beneath her left armpit, and his right arm over her right shoulder. The surgeon should be steady on his feet, and the patient should be asked to press back against him. He can thus regulate the pressure he is putting on with his hands to a nicety, and make the best possible use of his sense of touch. When the breast is examined from the front, the surgeon puts a certain amount of pressure on it, and the patient yields from it, and so the judgment of weight, so essential to determining the nature of a soft body, is lost. In the second position the patient should be seated in

a firm chair, and the surgeon should examine the breast from the front. In the third position the patient should be on her back on a sofa. Now, in each of these positions the surgeon will acquire a new appreciation of the tumour, but the first is by far the most effective, for in it he will avoid the mistake, so liable to be made in the other two (especially the third), of crumpling up a mass of sound tissue, so that it comes to feel just like a tumour. All things under the chin, down the neck, in the breasts, and in the groins should be examined by the surgeon standing behind the patient. In examining a hernia, for instance, the surgeon, having first restored the bowel, can, by standing behind the patient, constitute himself an animated truss, and can determine the exact spot where the truss-head will best control descent, and also judge the down-push caused by coughing, so as to estimate the power required for the truss spring. Students are not nearly enough impressed with the great value of this method of examination. It was Paget who first showed me the necessity of examining a breast in all three positions.

Acute Mania after Operations on the Breast.

A matter which is worthy of some little attention is the occurrence of acute mania after operations on the breast. That this occasionally comes on after any kind of operation is true, but there are some after which it is more common than after others. It seems to be only comparatively recently that it has been taken distinct notice of. Dr. S. Rutherford Macphail, of the Derby County Lunatic Asylum, has written in the 'British Medical Journal,' of September 23rd, 1899, p. 781, a short but eminently useful and practical statistical account of lunacy after surgical operations, from which I have extracted the following figures :—

His own experience of the malady embraces 900 admissions of all kinds to his asylum, of which 40 were post-operative, namely, 13 males and 27 females. Of the 27 women no fewer than 15 had been the subject of gynæcological operations, and of the remaining 12 five had been subjected to ophthalmic operations. Of the 13 male cases, four were after ophthalmic proceedings, and the rest after general operations of all kinds. All who have written about the subject apparently agree that the vast preponderance of cases occurs after gynæcological and ophthalmic

operations. Dr. J. Christian Simpson, in his work on 'Cerebra Symptoms relating to Surgical Disease,' speaks of a record of 185 cases by Sears, of which 50 were gynæcological, 62 ophthalmic, 45 general surgical, and 10 after excision of the mamma. Rianan out of 186 cases had 65 gynæcological and 35 ophthalmic. But, as showing how statistics often come utterly to grief, Dr. Denis (Montpellier 'Thesis,' 1889) reckons that out of all cases of operation 2·5 occurred in each 100. Dr. Simpson, however, proceeding to examine the records of operations of all kinds done in Professor Annandale's clinic from 1882 to 1894, found that out of 5,500 he could only find 11 cases of mental derangement, being about 2 per 1,000—a very different thing from Denis's figures.

In my experience I can only remember among general cases one male who became maniacal after operation. He had a large sarcoma removed from just below the axilla, and after about a week he became maniacal, and continued so for three weeks, after which he recovered and remained well for some years. There was nothing wrong with the wound here. One female died from acute mania after excision of an epitheliomatous ulcer of the rectum. But amongst my cases of removal of the breast I find eight in which acute mania supervened. Of these, two were maiden ladies between 50 and 60 years of age, and were women of excellent intellect and highly cultivated—both great readers. One of them was rather melancholic than maniacal, becoming so about a week after the operation, and remaining so for several weeks before the condition completely wore off. The wound united by primary union all through, and there was no rise of temperature. The other began to be queer in a few days after the operation, and for more than a fortnight talked very amusing and sometimes clever nonsense. In her case the wound broke completely down and suppurated profusely, because she was never still when awake; but it was never septic, and there was no rise of temperature, while the patient never became weak or looked ill. Both of these ladies are sound mentally as ever, one nine years after the operation and the other two years and a half. The next two cases occurred in hospital. Nothing especially went amiss with the wounds. One patient recovered completely, but the other never quite regained her mental condition, and ultimately died in a lunatic asylum.

Next I note four cases which all proved fatal, and I will add to them the female mentioned above, who died after the removal of a cancer from the rectum. Let us call them A, B, C, D, and E; E being the rectum case. To begin with, they were all private patients and in excellent circumstances. A's wound never became aseptic, but she became melancholic and died on the thirteenth day after operation. Her own mother and sister died from cancer, and she was the second wife of a gentleman whose sister and first wife died of cancer also. These depressing circumstances no doubt seriously affected the patient. Case B's symptoms appeared on the fifth day. I only saw her once after the operation, as she lived in another town, but her doctor sent me a very full report of her case. She exhibited extreme likes and dislikes, and one nurse had to be sent away because she would not tolerate her presence. Then she became violent and talked and kicked about incessantly. She jumped out of bed if not constantly watched, and once made her way into an adjoining room before she could be stopped. Curiously enough she allowed her wound to be dressed, and would even help in the process. At death it was practically healed. Her doctor had only attended her a very short time, but he subsequently learnt that she was a mysterious old widow who had married unhappily a second time and lived away from her husband under an assumed name. When well she had attacks of mental derangement, and there was insanity in her family. When her effects were being routed out a great quantity of spirit bottles were found secreted about the house, which showed that she was probably a secret drinker also. Cases C, D, and E were practically maniacal before they were operated upon. One was married and the other two were widows. They were all between 50 and 55, and were all noted as being flighty, singular persons, and very vain even when in good health. When they learnt that they were the victims of cancer their terror became indescribable. It was most distressing to have anything to do with them, and the interviews and consultations were very painful. On the day of operation it was noticed that they were all supernaturally calm and composed. Doubtless, this involved a great mental effort. In the course of a few days the wounds all broke down and suppurated profusely (although not in any way malodorous), while the temperature was high. They all talked, shouted, shuffled about unceasingly, and kept

wanting to get out of bed. One (rectum case) did escape from bed, and got on to the landing of the staircase. At times they would be quite collected and rational, and speak sensibly for a short period. They all took nourishment fairly. Now as the wounds suppurated, and as the temperature was high, one must admit that septic infection was present, and helped to cause their deaths; and it may reasonably be argued that the delirium was simply the delirium of septicæmia. But this was not the case. It began too soon after the operation, and before the wounds began to break down and suppurate. Moreover, in former days I have unfortunately seen too many persons die of erysipelatous and septic wounds, and in my experience they have never been violent and furious. They lay quietly muttering and moving uneasily; but they did not get out of bed, and run out on to the landing and into the next room. They had not the strength for that. To conclude this point, cases of post-operative mania have been often referred to the use of carbolic acid and iodoform. There seems no proof of this. Such toxic effects as may have occurred in this way have been transient in their duration, and have passed off at once on their ceasing to be used.

Chronic Mammary Induration: Chronic Interstitial Mastitis.

I would now like, gentlemen, to dwell with you for a while upon a subject of immense importance in connection with mammary cancer: I allude to chronic inflammatory induration or chronic interstitial mastitis. Billroth termed it chronic cirrhosing mastitis. There are sundry forms of this described. One is known as diffuse chronic mastitis. It is described as coming on with tenderness and swelling, without febrile symptoms, while the affected part becomes tumid and painful. The whole gland and even the overlying nipple may become involved. The affected skin has the appearance of being adherent, thickened, and rough looking (*peau d'orange*). Then, the inflammatory process subsiding, a cirrhosis takes place, the breast diminishes in size, and the nipple and skin are drawn in. The contraction being irregular, the breast becomes eventually a mere shrivelled, nodular structure. The axillary glands may also enlarge, and there may be a watery discharge from the nipple. Now I must confess that in all my experience I have never seen anything like this;

anything, I mean, so definite in its course and so startling in its results. So that surely such marked cases of chronic mastitis must be rare. Doubtless they occur, for the disease is described and the shrivelled organ figured by Billroth, but I think this form need not enter into our calculations. The most common form with which we are all acquainted is what pathologists call the "periductal" form—the *maladie noueuse* or plexiform fibroma of the French. Surgeons here call it *chronic interstitial mastitis.* It seems to be simply a very slow and painless variety of the preceding form, where the inflammatory process, confining itself strictly to the neighbourhood of the glandular tissue proper, results in a fibrous sclerosis. Bits of the gland may be affected or the entire organ. In the latter case the breast feels like a hard pancake which you can lift up from the subjacent great pectoral muscle. All through it are scattered hard masses from the size of a pea up to that of a broad bean. Besides these larger pieces there are little minute nodules distributed all through the gland. The microscope shows these to be small cysts, structures produced in all tubular glands under cirrhotic conditions. Sometimes only a portion of the gland is affected, while the rest is healthy and soft, but I see no reason for elevating such cases into a special class.

Thus, then, if a chronic mastitis continue to progress by developing more and more connective tissue, then what happens is the production of a cirrhosis with pressure destruction of the glandular tissue, with the formation of small cysts, and finally with completed shrinkage of the whole breast, this process being accompanied by the formation of nodules almost approaching the character of small fibroid tumours. If this be the bent of the mastitic change it is a happy thing for the patient, for no malignant state supervenes; but, as we shall see, the mastitis may progress by a more dangerous method, namely, by the immense proliferation of intra-glandular epithelium, which some day will burst its bonds and erupt into the surrounding connective tissue. This is cancer.

The disease is essentially one of the climacteric period, and in my experience seems to be as common in single as in married women. I have not been able to detect any connection between it and acute puerperal abscess, and, as bearing upon the production of cancer by injury, I cannot remember patients making

definite allegations that chronic mastitis arose from blows or traumatism of any kind. On the other hand, as I mentioned at my last lecture, the statement of patients concerning the direct connection of their cancerous growths with injury, and the presence afterwards of slight pain or uneasiness at the point thereof, have appeared to me to be worthy of more serious consideration than has been given them, and should not be dismissed at once as mere silly tales.

The principles of treatment of chronic mastitis by pressure, gentle friction, and the use of local remedies, such as used to be called "discutients," seems to be reasonable. I have a routine practice, which is distinctly serviceable, and which I imagine most practical surgeons employ also with variations. The patient is directed to bathe the breast for five minutes morning and evening with very hot water applied by means of a big cone-shaped sponge. Then there is rubbed into it mercurial ointment one week and liniment of potassium iodide with soap the next, the inunction being done with the greatest gentleness. After a while this treatment is apt to make the skin of the breast very tender and produce crops of painful little pustules. It should then be entirely left off, and soft zinc ointment smeared on until the skin irritation has subsided. Stays must be abandoned, and the breast covered with a layer of cotton wadding. Pressure is applied by means of a broad elastic webbing bandage. The first turn is brought round under the armpits and fastened with a safety pin, and then the patient can pull on the remaining turns as they go down over the mamma as tightly as she can bear, fixing it at the short ribs with another pin. The patient may take a moderate dose of potassium iodide morning and evening, unless it proves very disagreeable to her; but, after all, it is without doubt in the local treatment that confidence is to be placed. Of course I know that mercury and potassium iodide are very old-fashioned remedies; but I still have belief in them and in their efficiency as "discutients" of chronic, inflammatory indurations. But probably these simple views will not find favour with the practitioner of to-day, for whom a new drug has to be provided once a week from the pill and capsule blast furnaces of America and Germany.

Only a generation ago the universal teaching was that an innocent or non-malignant tumour remained so always, and never

changed its character, while a cancerous or malignant one was so from its very earliest inception, and never could be anything else. Surgeons have been compelled slowly to work themselves round to quite the opposite belief. It would almost seem with our present knowledge that, with the exception of simple cysts and pure fibromata or myomata, there is no neoplasm, however innocent in its commencement, which may not in time—perhaps after very many years—degenerate into malignancy. I have seen on two occasions even that most simple of all tumours—the lipoma—followed by what is called fat sarcoma, requiring two or three operations of most extensive character for its extirpation. Now, the degeneration of chronic interstitial mastitis into cancer is, of course, perfectly familiar to all operating surgeons. But I very much doubt if this knowledge is as thoroughly diffused throughout the profession as it ought to be, or rather, I should say, that I question whether medical men at large attach to it the immense importance which the matter deserves.

A little while ago I said I thought that attention might now be diverted from the devising of new operations for mammary cancer, for I believed that for the present they had in the majority of cases reached the limit of real use, and that the whole energies of surgical teachers should be thrown into the propagation of the gospel of early diagnosis; for there is no more difficult case in the range of breast surgery than to know what to do with a breast, obviously the subject of chronic mastitis, in which, among several very hard points, there are one or two which are of such special hardness that it is impossible to tell whether they have degenerated into cancer or not. My own practice is that if the preponderance—the distinct preponderance—of evidence makes for nothing worse than chronic mastitis, pure and simple, naturally the treatment above referred to should be at once put in practice, but even then the patient should be seen at regular intervals until all hardness is quite gone, for I have repeatedly seen the breast clear up from all induration—to the great delight of the patient—except at one spot, and there the cancerous limit has been reached and overpassed. When, on the other hand, the balance of probability is that the cancerous limit is being reached, even when to the touch no absolute certainty thereof is communicated, I have for many years practised and taught that removal of the breast is a justifiable proceeding. The way

I have done it is by making a semilunar incision along the lower border of the mamma, dissecting up the skin and nipple and laying them down again after the gland is removed, provided there is no sign that palpable carcinoma exists. How is this to be known? By cutting through any suspicious mass or nodule after raising the skin. I am quite sure that the section which an experienced surgeon makes with a knife through a fresh piece of carcinoma will enable him to give a far more decided and certain opinion than any the microscopist can give. Not for a minute that I would speak disparagingly of the value of microscopic examination. When it coincides with the naked-eye view of the clinician so much the better, but, when they differ, I hold that the history of the tumour and the look and feel of the fresh section constitute the most trustworthy guides. An error may always be made by taking a microscopic section from a limited portion of the tumour, where the process of growth is not actively going on. Moreover, there are inflammatory conditions of organs which so simulate in appearance certain malignant states, that the ablest microscopist will admit himself unable to give a positive opinion about them. But the colour, the resistance to the touch, and the feel to the knife of a fresh mammary tumour section, however small that tumour may be, are the most certain guides of all as to its nature. With regard to the retention of the nipple and areola, it must be remembered that they are to a woman the signs and symbols of the mammary gland, which is a distinguishing feature of her sex. There are many women whose breasts, when past middle life, are so small and shrunken, that but for the nipple and areola it would hardly be known that any breasts existed at all. I have an "impression" that chronic mastitis is more common in small breasts than in big ones, but it may well be said that this may arise simply from the disease being more easily detected in a small breast than in a big one. Still, having made a very great many naked-eye sections of cancerous breasts, the apparently sound gland tissue (away from the cancer) in a big, fat breast has rarely the tough hardness characteristic of a piece of chronically inflamed material. Referring again to the nipple and areola, if these be left to the patient she does not think so much of the loss of the gland itself. A man, similarly, will be quite content with a very wooden nutmeg kind of testicle, which is certainly useless functionally,

if only he be allowed to retain it in his scrotum as the badge of his sex. Assure the patient that her breast will not be materially disfigured, and she will generally consent to anything to get rid of the haunting dread of that terror of her life—mammary cancer. But what if there should be more than mere mastitis? When the skin is being raised any suspicious point or points should be cut into, and if any one of these looks cancerous, then skin and nipple, as well as the breast, can be swept away too, and the axilla cleared out, just as for an ordinary well-recognised case. About a month ago this very case happened to myself. Yet the patient's first shock of disappointment, when she learnt what had been done, was soon allayed by her satisfaction at feeling that she had been relieved of her enemy.

It is many years ago since I first proposed to the surgical profession that in every case of cancer, where the breast was removed in the hope of ultimate freedom from disease, the axillary glands should be removed as a necessary part of the operation. My argument was that it was impossible to tell whether they were involved or not, particularly in well-nourished persons. That argument has eventually prevailed over much opposition. And now to-day I would desire to impress upon the profession that it is a right and proper thing in many cases to remove breasts, the subjects of chronic mastitis, even before cancer has been definitely diagnosed in them. The question—and a reasonable one—at once arises, To what excesses may a doctrine like this not lead? What absurd and unnecessary surgery at the hands of the "enterprising surgeon" may not arise from this? Nay, may it not be a direct incitement to certain of the more needy class of surgeons to do operations for the sake of fees? I admit these possibilities, and yet I have such confidence in the good judgment and probity of the English surgeon, beyond all other surgeons in the world, that I believe the evil that may occur in this way will be far outweighed by the saving of life effected by the removal of nascent cancers. We know how absurdly overdone the removal of the appendix has been in some countries until it became positively the subject of public merriment; and yet, with all this abuse, many lives have been saved by it, while the operation is meantime quietly finding its proper level and sphere of usefulness.

I have seen several cases of chronic mastitis in men, and,

curiously enough, they have all been in private practice; one of the things that, I think, produces it in them is the rubbing of braces and brace buckles, and these, of course, must be at once removed and a belt worn. The course of treatment above mentioned seems to be perfectly successful. One case of this nature occurred in the person of a healthy young gentleman, who was going abroad to a distant part of South America, where he would be far from surgical assistance, and that of a very poor order. Some few months before his departure he developed a chronic mastitis, but did not take any notice of it, as he had no pain from it. But very shortly before he was about to leave the swelling increased somewhat, and he became very much alarmed in case anything dangerous should occur to him abroad in his out-of-the-way location. I told him there was no time for any treatment, and that the gland, which was a mere rudimentary and useless affair, could be removed with the certainty of a cure in a week. The removal was done by the semilunar incision, preserving the skin, areola, and nipple, so that no disfigurement resulted, and he went away feeling quite happy in his mind.

At this stage the question naturally arises, If the transition from chronic interstitial mastitis into cancer is a thing whose occurrence is admitted by all surgeons, can we guess in what proportion it occurs? Here it seems as if statistics were of no avail whatever. What attempts have been hitherto made have lost weight by reason of the fact that they have included all kinds of mastitic trouble. Now, my contention is that the mastitis out of which or in which carcinoma develops is of one kind, and of one kind only—namely, the chronic interstitial mastitis, which arises spontaneously about the middle period of life, and has nothing to do with any previous inflammatory condition. Putting aside infantile and adolescent mammary abscess, the only other acute inflammatory condition which arises in the breast of moment is milk abscess. It occurs in women during the active child-bearing period between 18 and 35. When opened, drained, and cured, all traces of inflammatory induration disappear in their sites. I have seen breasts the subjects of "rabbit-warren" milk abscesses, which I have literally cut in pieces, but which have healed so remarkably that the scars of them have been no great disfigurement, while the breast tissue has been as supple and soft as if nothing had been wrong with it.

Again, in examining all kinds of female breasts for all sorts of trouble, I have found the scars of incisions few and far between and of a very simple nature. Mr. Bryant, in 'Diseases of the Breast,' says:—"My own statistics tell me that out of my 360 cases of women who had borne children mastitis had occurred at some antecedent period in 80, but it is almost impossible to make out that the cancerous tumour originated at the seat of scar or induration. The tumour in the majority of cases appeared many years after the mastitis, in one case 30 years."

I cannot, therefore, agree to the statement, copied from one book to another, that acute milk abscesses leave behind them permanent indurations which degenerate into carcinoma, or that the scars made for their relief are similarly dangerous. Chronic interstitial mastitis of the menopause—*voilà l'ennemi!* My friend and colleague, Mr. Paul, has long studied this question, and has an unrivalled collection of specimens illustrating it. It is from his stores that the most beautiful of the specimens which I shall show you to-night have come, and I hope that he will lay before the profession some day the results of this histological work. I would wish publicly to thank him for his courtesy, and also my friend Dr. Buchanan (whose work on the 'Cancer Parasite' will come into my next lecture) for the trouble he has taken in arranging and preparing the lantern demonstration for to-night. Mr. Paul, like myself, while unable to fix a percentage, believes most firmly that a much larger number of cases of chronic interstitial mastitis are allowed to drift into cancer than is commonly believed.

The suggestion has been made and carried into practice by not a few surgeons that, where there is a doubt about the nature of a hard piece of chronic mastitis, the patient should be anæsthetised, and a piece of the tumour removed and subjected to microscopical examination. I have done this twice, but it does not seem a satisfactory proceeding, although theoretically it appears a very sensible thing to do. The truth is, that what the patient dreads is chloroform and an operation of any kind. If the microscopist declares in due time that the disease is malignant, then another chloroforming and another operation have to be faced. To everyone, and more especially to a woman, it is the prospect of a surgical proceeding that terrorises, and, however much we may tell our patient that the removal of a small piece of her breast for

examination is a mere trifle, to her it does not appear in that light, for she is no judge of the comparative severity of operations. In my first case I felt practically sure that the lump was cancerous, but the relatives were in doubt, and would not allow removal until satisfied by the microscope that it was so. As the microscope did pronounce it cancerous, the breast was duly removed, and the patient had all the anxiety of the exploratory operation for nothing. In the second case the microscopic sections were so dubious in character that they were practically of no help, and the diagnosis by the hand was just as good. It does not seem as if the exploratory examination will become popular. The best way is to tell the patient and her friends the true state of the case and the sources of doubt, and to ask permission to proceed as you think fit on the strength of a naked-eye section made under an anæsthetic.

It is universally admitted that the essential feature of carcinoma consists in the escape of epithelium from the interior of the alveoli into the surrounding connective tissue and its subsequent proliferation there, this cancerous epithelium differing but slightly from the normal variety. It is clear that the ultimate reason for its proliferation must always be incomprehensible to us, involving as it does the principles of life and growth. Gland epithelium is always growing and always perishing, and always being eaten up or discharged. So long as the balance between growth and waste is even, there is health; when growth exceeds waste, there is neoplastic disease. Apparently, as soon as the epithelial cells escape from their normal habitat and get into the spaces that exist between the connective tissue fibres, they increase at a great rate. Why? This is the crux of the whole question. Some hold that the irritation produced by parasitic bodies excites the growth of epithelium in ultimate alveoli or tubules, and carries on the excitement when the epithelium has burst its bounds and escaped into the cellular tissue.

In the 'Journal of Pathology,' August, 1899, is detailed a very remarkable experiment by Dr. Lack. His view is that there is no necessity for such a view as a parasite, and that carcinoma is simply the result of the entrance of the normal epithelium of the body by some means—probably traumatic—into the lymphatic spaces, and its continued growth therein. The peritoneum of a rabbit was opened, the ovaries cut open, and the milky juice from the cut surfaces allowed to diffuse through the peritoneal

cavity. An opinion was received from Professor McFadyen that the rabbit never developed cancer spontaneously in his experience. Thus normal epithelium cells from a healthy animal were introduced into its own lymphatics. The rabbit remained well for nearly a year, after which it became thin and weak and suffered from dyspnœa. It was killed at the end of 14 months. White, hard, round nodules were found in the mesentery. Great masses of tumour-like tissue were found all about the thorax and abdomen, so that the spleen, kidneys, and one or two other organs were alone found to be normal. Sections showed "the typical structure of a columnar-celled carcinoma." Alluding to the fact that malignant disease of the peritoneum has been known to follow the removal of ovarian tumours, the fluid of which contains free epithelium, Dr. Lack does not admit the explanation that these tumours were necessarily malignant.

Presently came a counterblast from Mr. Shattock, who showed to the Pathological Society of London* a rabbit whose uteri were full of cancer, while the peritoneum contained growths also, this animal not having been the subject of any experiment. Why might this not have been the case with Dr. Lack's rabbit? He notes also that, after the rupture of an ovarian dermoid, multiple cysts may form in the peritoneum; but these are simple circumscribed cysts, and not malignant growths, such as should arise were Lack's hypothesis of the mechanical origin of carcinoma in the displacement of normal epithelium into a lymph space or lymph cavity correct. In this condition we must leave the matter for the moment, acknowledging the almost startling nature of Lack's experiment, but feeling that we cannot trust to a solitary example.

Let us see, then, if it be possible to trace the gradual development of a piece of carcinoma in a chronic interstitial mastitis. The earliest change probably consists in the appearance of quantities of leucocytes, which constitute a round-celled infiltration distributed in the neighbourhood of the ultimate lobules. The single-layer epithelial lining of the tubules and acini which compose the ultimate lobules then begins to proliferate and grow into the interior of these cavities; at the same time there is a production of a new and very delicate connective tissue, which surrounds the tubules and acini of each ultimate lobule. The normal connective tissue outside of that also increases largely in

* 'Lancet,' April 29th, 1899.

bulk; by reason of all which things many tubules are so squeezed and strangulated that, their distal portions being cut off, there is no exit for their contents, and they therefore become distended, and form the innumerable little closed cysts which are found all through the cirrhotic breast of chronic mastitis. Matters may remain thus for long enough, the breast shrivelling more or less. This is the cirrhotic change already alluded to; but another change may occur. The growth of the epithelial cells in the tubules and alveoli may go on to such an extent that they become completely blocked up, and appear as black areas on microscopic section. The fine interacinous connective tissue does not increase much in this case, but the ducts and acini, becoming more and more distended, eventually meet and fuse into each other, so that in place of a number of small black areas we now see large ones, still giving slight evidences of the agglomerated acini of which they are composed. These masses of cells, however, are still held together by the surrounding interlobular tissue and by the limiting membranes of the original ducts and acini; but finally even these give way, and the cells pour out into the strata of the connective tissue. They creep along between the bundles of connective tissue, following the tracks of what are probably the very earliest commencements of the lymphatic system, rapidly reproducing themselves by the process of karyokinesis. They are now cancer cells, although between them and the original epithelial cells lining the tubules and alveoli there is not a great difference. There is certainly nothing absolutely distinctive. The normal glandular epithelial cell is of very regular shape and size, and has one well-marked nucleus, being an orderly-growing cell. The cancerous epithelial cell varies a good deal in size and shape, being a young, rapidly-growing, erratic cell. There is some difference in the way in which certain stains affect, with more or less intensity, the cytoplasm of the two kinds of cells, but this is not improbably due to the difference in their age. Mr. Plimmer notices that in all actively-growing cancers there is a zone of round-celled infiltration at the margin of the tumour where growth is most intense, and this zone does not exist in simple tumours. Near the more central parts of the tumour these round cells are converted into large mononuclear cells, and later on into fibrous tissue, which surrounds the cancer cells. This stroma next causes a degeneration of the cancer cells—a digestive or

phagocytic process. As time goes on the stroma hardens, and serves the function of protection against invasion, so that the centre of a carcinoma is the hardest and most fibrous, and thus comes to pass the fact also that the harder the tumour—in other words, the more fibrous tissue there is in it—the less is the chance either of generalisation or recurrence. So, too, we arrive at the explanation of the atrophic cancer, where the cells have been really all devoured by the connective tissue.

In the 'Transactions of the Pathological Society of London' is a very interesting paper by Mr. Cecil F. Beadles, which has not been accorded the prominence it deserves. It is on "Certain Histological Changes found in Association with Glandular Carcinoma." He examined microscopically nearly 100 breasts removed for primary carcinoma. He came to the conclusion that breasts in which such tumours are found are never in what may be called a normal state. It is often possible to make out with the finger small hard nodules situated at a distance from the tumour, and having no apparent connection with it. Sometimes, however, only slight thickenings can be felt in some parts of the breast. The acini show proliferative and other changes in the epithelium, and there are to be noticed also changes in the stroma, which comes to resemble that seen in some rapidly-growing scirrhous tumours in which connective tissue cells with their elongated nuclei run parallel around the acinus, having between them what appear to be small uniform cells with round nuclei. The changes are both of the nature of chronic interstitial mastitis, and, in the case especially of the thickenings and small nodules, of a condition intermediate between simple adenoma and carcinoma extremely suggestive of commencing malignant disease.

Mr. Beadles is of opinion that these changes in the apparently sound breast tissue are not produced by infection from the existing carcinoma, but are most likely due to the same causes which started the tumour. Clinically, and quite apart from all microscopic examination, this is the opinion I have always held myself. In plain language, it means that there is a precancerous state of the whole breast, which, however, only blazes up into full-grown cancer at one or two points. It was this view which made me utterly disbelieve in the removal of the tumour alone, and which prompted me many years ago to press vigorously upon the attention of the medical profession the advisability of free removal of the entire breast and with that the clearing out of the axilla.

FIG. 1.

Fig. 1 represents chronic interstitial mastitis affecting a lobule. The lighter part with the dark areas in it is the lobule; *a* shows tubes and acini of a lobule filled with proliferating cells, but they are being compressed by the growth of a young, fine intralobular connective tissue, *b*; *c* is the older interlobular connective tissue of the breast, also much hypertrophied (× 50).

FIG. 2.

Fig. 2 shows an advanced hypertrophy of the connective tissue, so that the glandular tissue is almost squeezed out of existence; *a*, atrophied gland tube and acini; *b*, large duct partially filled with cells, with an empty one close by; *c*, hypertrophied connective tissue (× 50).

FIG. 3.

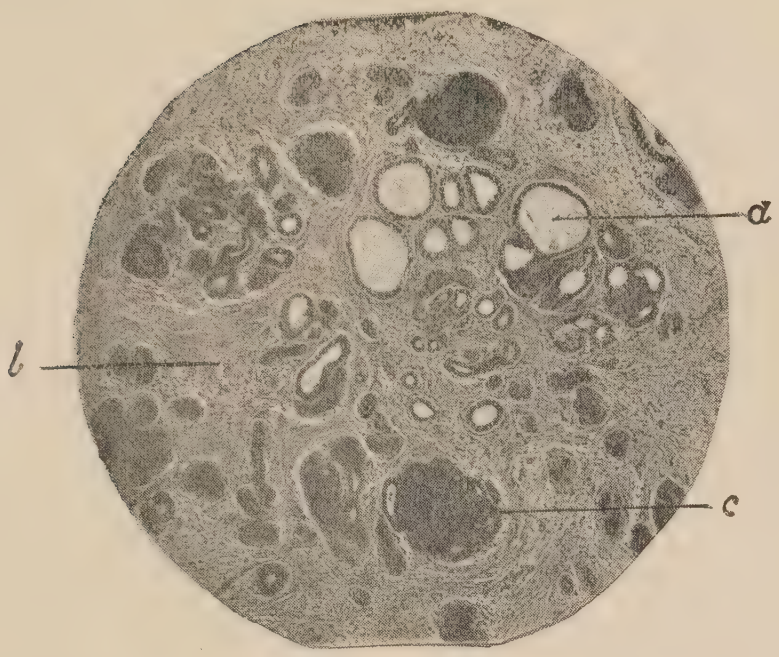


Fig. 3 at upper part shows ducts and acini dilated owing to the compression by connective tissue at other parts. These produce the innumerable small cysts found in chronic mastitis; *a*, dilated ducts and acini; *b*, hypertrophied connective tissue; *c*, mass of acini, with others close by, so dilated by proliferating epithelium as to press against each other and practically become fused. This lower part might become dangerous some day (× 50).

FIG. 4.

Fig. 4 shows mastitis degenerating rather towards cancer than towards cirrhosis; *a*, old interlobular connective tissue, considerably hypertrophied. The centre of the figure is occupied by the greater part of a lobule. There is no great overgrowth of young, interacinous, connective tissue. At *b* the acini are still separate, although filled with proliferating epithelium; *c* shows a large mass of cells formed by acini, which have swelled out under cell pressure, have met and then fused. The white holes from which cells have dropped evidence the sites of the former acini. Cells still well limited (× 50).

FIG. 5.

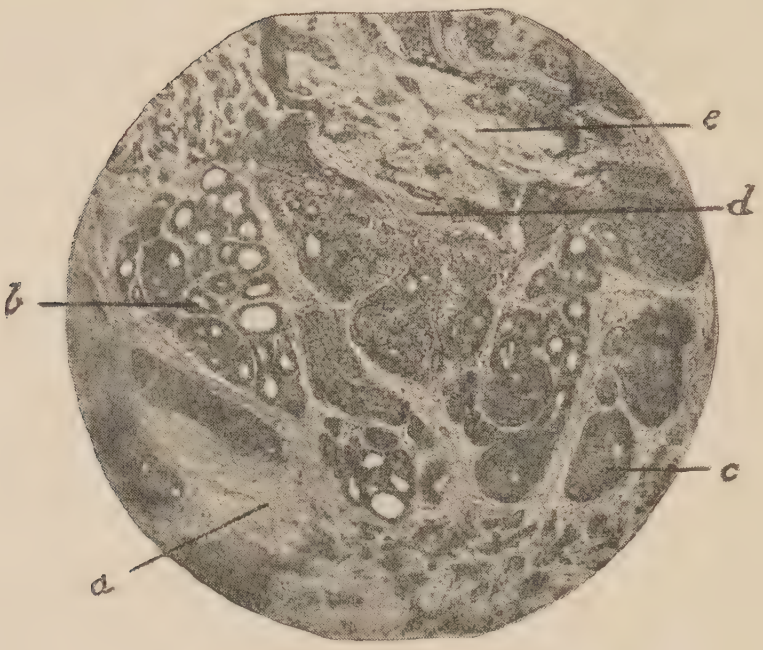


Fig. 5, mastitis becoming cancerous; *a*, connective tissue; *b*, a lobule with acini, filled with proliferating epithelium, of which some has dropped out. Not much interacinous connective tissue; *c*, masses of cells composed of fused acini, still well defined; *d*, cells from such a similar large mass bursting into the connective tissue and becoming cancerous at *e* (× 50).

FIG. 6.

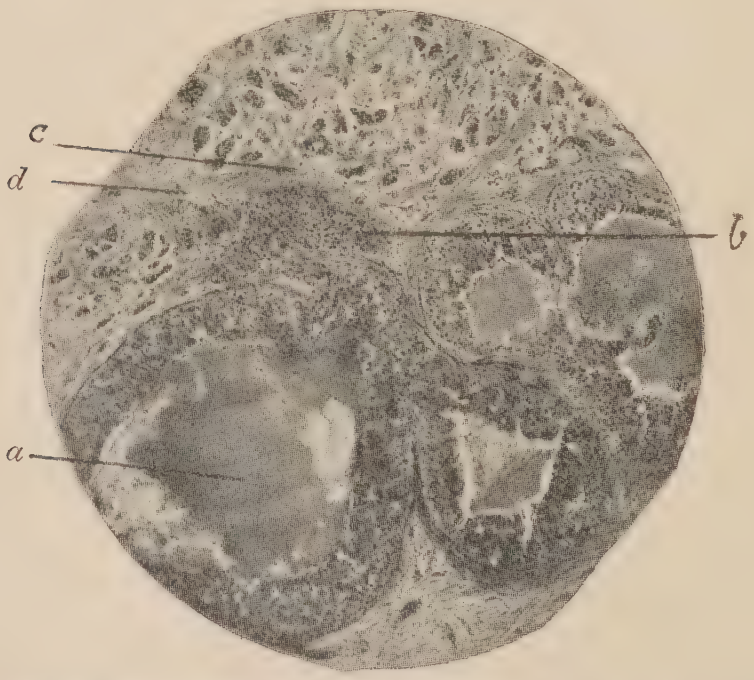


Fig. 6, mastitis become cancerous; *a*, large fused mass of acini, with two neighbouring ones; central cells have degenerated, and some have fallen out; *b*, cells escaping from this mass into surrounding connective tissue; *c*, a large mass of these escaped cells; *d*, true cancer, irregular cell masses now running in all directions through the connective tissue strata (× 50).

LECTURE III.

The Parasitic and Infective Views of Cancer.

It would be out of place here to enter into a detailed history of the early searchings after truth in this matter, for, however praiseworthy, nothing definite came of them. The first notable step in advance was made by Dr. Russell, of Edinburgh, in 1890. In the 'British Medical Journal' for December of that year will be found a paper by him modestly entitled, "A Characteristic Organism of Cancer." Seeing that they were well displayed by staining with fuchsin, he termed them "fuchsin bodies." The attaching of men's names to all sorts of silly little discoveries and operations is much to be deprecated, but so important a move in the right direction might well be commemorated by designating them Russell's bodies. He described them both as intracellular and extracellular, and, what is of much importance, he held them to be blastomycetes, a variety of the saccharomycetes or yeasts.

The general opinion was distinctly against him, but in the 'Annals de l'Institut Pasteur' for 1890 appeared a paper by Professor Soudakewitch of Kieff, "Parasitisme Intracellulaire des Néoplasies Cancéreuses," describing and figuring bodies obviously very similar to Russell's bodies, and in the same year ('Journal of Pathology') a very important contribution by Armand Ruffer containing confirmatory evidence. Preparations illustrating both these papers were shown to Metchnikoff, and that distinguished pathologist gave it as his opinion that the bodies were "parasitic protozoa." This at once altered the *venue* from the vegetable to the animal kingdom. In this country the fuller investigation of the parasite has been very ably carried on by Mr. Plimmer, of St. Mary's Hospital. In the so-called Cancer Number of the 'Practitioner,' already referred to, will be found a review by him of the whole question up to April, 1899. It is a model of clear unbiassed writing, and the description of the general structure of cancer and methods of examination most admirable.

For a considerable time, my friend and old pupil, Dr. Buchanan, formerly Assistant Demonstrator of Physiology in University College, Liverpool, and now Physician to the Liverpool Consumption Hospital, has been investigating these parasites, and

he recently (January, 1900) contributed a paper upon them to the Pathological Section of the Liverpool Medical Institution. I have had the pleasure of going over all his work with him, and I cannot help coming to the conclusion that the bodies described as parasites are bodies *sui generis*—special organisms—and are not degenerations of cell protoplasm or of non-parasitic cell inclusions. It seems as if the almost daily increasing evidence is now too strong to permit of our simply listening to those sceptical persons who merely deny their existence without having ever hunted for them or even seen them. Where these bodies come from, where they go, or what they do, is nothing to the point for the moment. The question is, Do they exist? I have been convinced that they do.

A very reasonable question on the part of the doubter is this: Cannot these said parasites be found in structures other than carcinomata and sarcomata? After Russell's pronouncement, many investigators said that this was so, but, as time has gone on, they have failed so far to prove their case. On the other hand Plimmer's most extensive inquiries have gone all the other way. During the six years preceding 1899 he had examined microscopically 1,278 cancers, taken as they came from many parts and organs, and of all possible varieties; out of this number he has found the parasite in 1,130 instances; of the cases in which he failed to find them, 63 were densely fibrous and atrophic, actually in process of spontaneous cure, and 23 were so degenerated that they would not stain. He has examined by his method gummata, typical histoid tumours of all kinds, tuberculous growths, glanders, actinomycotic growths, epithelium which had been artificially irritated, and normal tissues; and never in man, in any instance, has he seen any intracellular or extracellular bodies like the parasitic bodies he describes or which had their reactions.

The opinion of Metchnikoff that the parasitic bodies were protozoa was for a while the dominant one with probably all investigators. But Russell's view that they were blastomycetes has been revived by two eminent Italian *savants*, whose names are at present in the mouths of all who are interested in the question. One of these is Professor Sanfelice, Director of the Institute of Hygiene in the Royal University of Cagliari, and the other is Professor Roncali, of Rome—the former working largely from the

experimental point of view, and the latter from the histological. Now, their opinion is distinctly that the parasites are blastomycetes. Plimmer, however, still holds to the protozoal view as the better for provisional acceptance until more is known. It is clear that the question is not a vital one, and that, at all events, it does not in any way hinder the experimental work now going on, which has for its object to prove that these bodies, when they are separated from the other constituents of a cancerous growth, and when they are cultivated and the cultivated products implanted in animals, will be reproduced in these animals and originate cancer in them.

What, then, is the appearance of these parasites? As I have seen them in Dr. Buchanan's sections they are round cells, possessing what would correspond to a central nucleus, a surrounding mass of indefinite protoplasm, and an enveloping cell wall or enclosing capsule of some kind. These general features have been brought out by different investigators in different ways, but the plates published by them show a great uniformity in the general appearance of the parasite.

The method of display pursued by Dr. Buchanan is this. As fixing reagents he uses (1) a saturated solution of perchloride of mercury, in 0·75 salt solution, in which the specimen was placed for 24 hours, and then transferred to methylated spirit to which had been added a few drops of saturated solution of iodine in alcohol; (2) bichromate of ammonia; (3) Fleming's solution. As regards staining processes he found the Ehrlich-Biondi triple stain very good, but apt to fade too quickly. Another method: Stain with Ehrlich's acid hæmatoxylin, wash in distilled water, then "blue" in distilled water rendered faintly alkaline with lithium carbonate. Again another: Stain with saturated aqueous solution of fuchsin S. or carbol fuchsin S., then dehydrate in absolute alcohol strongly tinged with aurantia, clear in clove oil tinged with aurantia, and mount in xylobalsam. The size of the cells is very various. Plimmer reckons them at from 0·004 mm. to 0·04 mm., or even more. I have seen some which have a circumferential row of fine dots disposed just within the capsule, and others in which the nucleus has an appearance of being split up and the portions somewhat twisted. Again, there are some which have most delicate lines, radiating from the centre to the circumference. Some writers have described and figured

quite an array of different forms of the parasite, but personally, possibly owing to my want of skill, I have not been able to make certain to myself of more than the varieties mentioned above. As far as regards their mode of reproduction, Plimmer is of opinion that it is by division into two by the process of budding; or the nucleus divides into two while the capsule throws in septa which meet between these two halves, and so divide the parasite into two. Dr. Buchanan thinks they spore, but in his specimens I have not been able to make sure of anything definite as regards this point. I think Plimmer's immense experience makes his opinion the one to be received, at the moment at all events. In a recent interview which I had with him, he most kindly showed me his specimens, and gave me the very beautiful one which I show you to-night. Those of you who may care to turn back to his article in the 'Practitioner' will see that Fig. 1, Plate I, represents this in the most faithful manner. I also saw specimens identical with Fig. 8, Plate II, where the nucleus, becoming somewhat oval, had then divided into two.

I would like here to quote some of Dr. Buchanan's conclusions :—

"By careful comparison I found in these bodies a constant recurrence of definite structure, a beauty and regularity of design marked by stages of enlargement and growth with many evidences of a truly developmental process. Degenerative processes are essentially irregular, with no definite recurrence of form and with no beauty of design. I entertain two ideas :—(1) That if these bodies were truly parasitic from without, two conditions were necessary for their entrance into cells: (*a*) either they must be capable of movement constituting change of locality, or (*b*) the (epithelial) cell must be capable of movement sufficient for it to enclose the parasite, or a combination of both conditions at one time. (2) That, as a cancer is progressive in growth, the parasite must develop *in situ ;* if it do not, then, if it be an exciting cause, the growth must eventually cease."

He then goes on to express his opinion that the parasite is capable of movement, and that its appearance in many cases is due to definite change of shape, such as is generally recognised in amœboid cells ; that in its young state it enters a cell and there develops, dividing and redividing into a number of spore-like bodies, which may eventually fill and swell the capsule into a cyst,

in which the spore bodies lie like a mulberry mass, and the cyst resembles a sporocyst; that these spores eventually find exit from the capsule, probably through a special aperture, whence they infect other cells; that the parasites are properly fertilised to ensure reproduction, although the method has not yet been demonstrated; that they are living entities introduced from without to which the cancer cell occupies the position of a host.

It has been matter of wonder why, if the parasites are such well-defined bodies, they have not been noticed long ago. But the fact is that in any average tumour they are comparatively few in number and have to be searched for, and (a most important point) they are not to be found in the hard centre of a scirrhous mass at all, but only at the growing edges of a tumour. The centre is largely composed of fibrous tissue and cells in course of degeneration. That is the inactive part of the tumour, where nature is making an effort at cure. All the vigorous proliferating epithelium is at the margin, and it is only inside active cells that the parasite is found. Then the processes of staining required to display the parasites are very delicate and complicated, and are really only the product of the last few years.

Ignoring, then, the question of whether the parasites are to be considered as yeasts, and to be named blastomycetes, or whether they are protozoa or coccidia, has anything experimental been done with them? I will begin with some recent observations made by my friend, Mr. Keith W. Monsarrat, Assistant Surgeon to the Liverpool Infirmary for Children; made in order, if possible, to confirm the observations of Sanfelice, Roncali, and others. Having obtained a fresh cancerous tumour, he made into its outlying parts a series of some 20 or 30 incisions with a sterilised knife. Scrapings were taken from the edges of these incisions and inoculated on glucose-agar. From this subcultures grown on wort-agar and wort-bouillon were employed, and were plentiful after 24 hours' incubation at 37° C. From this again guinea-pigs were inoculated by intraperitoneal injection of 1 c.cm. of a culture 48 hours old. They showed no symptoms of illness, and were killed at periods from two to six weeks after the injection. On opening the abdomen the omentum and general peritoneal surface were found to be studded with nodules from the size of a pea to that of a pin's head; to the naked eye nodules were visible in the lungs, liver, spleen, and kidneys. The parasites were found

reproduced in the cultivations and in the inoculation growths. Mr. Monsarrat employed the following staining process for them, which he believes very accurately differentiates them and picks them out from surrounding structures. Fix with saturated solution of perchloride of mercury and harden in alcohol; place in acetic carmine for half an hour and then wash in water, followed by two minutes in methyl violet 1 per cent., and two minutes in picric acid 25 per cent., then transfer to clove oil until no more methyl violet will come off. The primary growths on the peritoneum are composed of proliferated endothelial cells. The organisms are present in considerable numbers, some within the cells, but most outside them. This is contrary to what occurs in primary cancers in the human body, where the extracellular parasites are few in number compared with the intracellular. In many of the nodules there is some attempt at the formation of a connective tissue capsule, and in others the central parts are broken down. In the lungs the nodules are made up of endothelial cells; in each nodule there are organisms present, and usually centrally situated. In the liver and spleen the nodules are very similar in appearance to the primary omental growths. The origin of the cells composing them is doubtful. In the kidney, where again the nodules are of endothelial origin, the cells are derived from those lining the Malpighian corpuscles and tubules. In no case was there any alveolar arrangement of the cells or any appearance resembling the endotheliomata of man.

With regard to the cultures, the appearance agreed in the main with those described by Sanfelice and Plimmer. Fresh specimens from them are spherical, from 4 to 10 microm. in diameter, and in most instances take an aniline chromatin stain diffusely.

I think we may regard Mr. Monsarrat's work as quite confirmative of what Mr. Plimmer has detailed with such admirable clearness and precision in the Cancer Number of the 'Practitioner.' There his own experiments on the same lines are to be found, and there also the fullest directions for staining, according to the method which he has found the best, and which produces specimens at once beautiful and convincing. To anyone who wishes to know about the parasites of cancer this article is invaluable. But what has been briefly touched upon in this description regarding the results of the inoculation of animals with the parasite has been far outdone by Sanfelice, who began his researches in 1894, and

whose last conclusions were published in 1899. Dr. Russell has written a most admirable summary of Sanfelice's last experiments.* If you will allow me, I will give you the barest outline of them. Sanfelice was investigating the characters of blastomycetes, when he found that some of them were pathogenic to animals and caused the growth of tumours in them. He selected a special organism for purposes of experiment, which he denominated the "saccharomyces neoformans." Inoculations of guinea-pigs with it produced a general infection of the animal with the formation of tumours, consisting partly of masses of the parasite and partly of proliferation of the cells of the part. The general microscopic appearance of the new growth seems to have been pretty much what Plimmer has recorded as the result of his experiments, and what Monsarrat has described as produced in his, as we have just noted. Bodies were perceived having the appearance of what the earlier investigators described as coccidia (protozoa), and what Russell had described and regarded as blastomycetes. These bodies as found in the tumours differed somewhat from those in the cultures as regards appearance. He inoculated with pure cultures of the saccharomyces neoformans certain cats, and found typical forms of Russell's bodies in the tissues. To make sure of the matter, he investigated the tissues of normal cats, but no parasites were found it them, although this occurred invariably after the injection of the special blastomyces. The cats, in short, were very susceptible. Cats, by the way, are liable to cancer, although not nearly in the same proportion as geldings. Next, the experimenter proceeded to pass the blastomyces with which he was experimenting through a number of dogs—that is, from dog to dog. In two instances something very specially tending towards malignancy was produced. In one case the dog was inoculated in the two posterior nipples with the blastomyces which had been passed through several dogs. The animal died 10 months later with tumours in both teats, showing retraction of the nipples and tacking down of the skin, with secondary growths in the inguinal and abdominal glands. All these showed the type of adeno-carcinoma. In the other case, a dog which was inoculated in both testicles died with adeno-carcinoma of these organs, with secondary growths in the neighbourhood of the penis and glans. If all this be true, it will constitute a remarkable

* 'Lancet,' April 29th, 1899.

advance in the history of the parasitic view of cancer, more especially when it is considered that these experiments were conducted with cultures made from an organism obtained from a fruit juice. These he found much more active than the cultures made from cancer. I have seen a microscopic section of a tumour produced after passing a blastomyces through many animals, but I confess I could not recognise in it a typical carcinoma, although it assuredly tended in that direction, and had a certain amount of connective tissue in its composition.

If I were now asked what is the impression produced on my mind by what I have seen and read about the so-called Russell's bodies or cancer parasites, I would put it briefly thus:—(1) There is plainly a structure of a definite nature which is found in man at the marginal or growing edges of carcinomata. It is not found in any healthy tissue nor in any other neoplasm except sarcoma. It used to be regarded as a protozoon or coccidium, but most investigators seem now to regard it (as Russell did at first) as a blastomyces which is a form of saccharomyces, reckoned to be a development stage of certain fungi. Most parasites are inside the epithelial cancer cells; some are outside. There is a strong probability that they spread by sporing, although much more knowledge on this point is wanted. How they get into or originate in the human body is not known, and what action, if any, they have upon the epithelial cells has not yet been demonstrated. But, admitting an action, it is doubtless that of an irritant to them, which causes them to proliferate. (2) There is no doubt that from these parasites cultivations in certain media can be made, and that, when such are injected into animals, growths are produced in them, mostly of a fatal character, which contain these parasites. (3) Sanfelice asserts that he can produce the same results in animals by the injection of a blastomyces obtained from a fruit juice. Furthermore, that by passing the parasites through a series of animals a tumour is at length produced which is an adeno-carcinoma. On these points I cannot say anything of my own knowledge beyond what I have said a few sentences back regarding a section of such a tumour which I have seen. (4) That as yet there is no clear and indisputable proof of the infectivity of cancer; that consequently any strong assertions to that effect, or any statements that surgeons infect wounds by disseminating cancer products through them during operations,

are not justified. That, on the other hand, the possibility of this infectivity must not be sneered at or treated lightly in view of the remarkable information which has been obtained during the last few years concerning the cancer parasite and certain of its attributes. This is not the time for positive assertion. This is the time for patient experiment and the collection of facts.

Operative Procedures in Mammary Cancer.

When I left my medical school in 1862 my recollection of operations for cancer of the breast was that they consisted of local removals of the tumour of the most imperfect description. If any glands were affected, not merely in the case of breast cancer, but in all instances, *ipso facto* that was a sufficient bar to any operation. In 1867 Mr. Moore, of the Middlesex Hospital, whose experience of cancer in that institution was great, wrote what is now an almost historic paper on 'Inadequate Operations in Cancer,' which set a great many minds thinking, inasmuch as it was a revolt against constituted authority. It made me think too, so that I became deeply interested in the question. I hope that in what I am about to say I may not appear to be too self-satisfied with what little share I have been enabled to take in it. But this does seem an opportune occasion for presenting to the medical public a review of the history of the operations for mammary cancer, seeing that the changes that have occurred in a very brief period have been of profound moment, and what has been done only a few years ago seems wonderfully soon forgotten. Well then, during the 10 years following Mr. Moore's protest, great progress was made by the persevering and bold manner in which Lister and Bryant and Annandale and Heath and other able surgeons attacked secondary glandular growths, and demonstrated that all that was required for their safe removal was careful, patient dissection. Among English-speaking surgeons Samuel Gross, of Philadelphia, was the next who made a notable move in favour of the free removal of breast cancers,* and to him much is due for the earnestness and enthusiasm with which he pushed his cause, as will be remembered by all those who, like myself, had the privilege of being his friend.

My first paper on the subject was read to the Lancashire and

* 'Tumours of the Breast,' 1880.

Cheshire Branch of the British Medical Association in 1877, and was termed 'A Plea for the more free Removal of Cancerous Growths.' In it I said:—

"I can point to several cases of hard scirrhus of the breast in which the whole mamma has been removed which have now remained well for years. There is no mystery about this; all that is required is to take the precaution to cut freely through sound textures. But this is exactly what surgeons will not do. I have over and over again watched a surgeon remove a breast, and have seen him make his incisions so as to include a little elliptical bit of skin containing the nipple, after which he would proceed to scoop out the breast so as to get nice flaps, which fell easily together and covered in the wound. He never considered that, in the brawny dotted skin over the tumour were already implanted the germs of the disease, and that the most dangerous thing he could do was to leave plenty of skin; one of the tissues above all others along which cancer cells love to make their way. I can safely affirm that in the great majority of operations for cancer which I have seen—and they are not a few—the cancer has not been removed root and branch. When the tumour has been carefully looked at after the excitement of the operation has been over, there has generally been found some spot where everyone wished that a bit more had been taken away. Not long ago I was looking over some old illuminated manuscripts. Among other pictures of the tortures inflicted upon the faithful there was one in which a horrid wretch was represented with a great crooked knife mangling the body of a devout female. He had slashed off one breast, the former site of which was represented by a great bleeding chasm, while with one hand he was clutching the remaining organ, and with the other was preparing with one sweep to lop it off also. I could not help wishing that I had this picture to hang up in the students' room in our school; not *in terrorem*, but as my idea of what most amputations of the breast ought to be; not a niggling, half-and-half digging out of the tumour, but a radical sweeping away of the whole mamma, skin and all. . . . . I have a very strong notion that it would not be a bad plan when removing a breast always to clear out the axillary glands whether we can feel anything wrong with them or not, and I mean to try it. At present we only remove the

glands when we can distinctly feel them enlarged, and that frequently through a great deal of fat and a thick skin. How do we know they are not already infected? And then the patient disappears, and doesn't return till the axilla is full, and the great vessels are so mixed up with the disease that we despair about being able to clear everything out and so leave her alone. . . . . By disabusing the lay mind of the unreasonable dread of operation, the public will come to regard an operation for cancer in the same light as they do an operation for stone or for cataract; a bad job at the best they will say, but still with a prospect of ultimate cure. They will come to us when their cancers are small and request us to remove them, and, if we could only extirpate them freely, what might we not expect? Have you ever imagined what the results would be if all cancers were thoroughly excised when they were no bigger than peas?"

Twenty-three years have elapsed since I said the above words, and I have very little more to say now.

In 1880, three years after the above appeared, Mr. Samuel W. Gross's admirable work on 'Tumours of the Mammary Gland' was published. In it occur the following sentences:—"This point demonstrates that the wiser course is to explore the axilla, even if its glands cannot be detected from without. By clearing out that cavity in all operations we may naturally expect to diminish, if not prevent, further local dissemination, and remove foci of general infection."

My next papers were read before the meeting of the British Medical Association in Worcester in August, 1882 (on "Free Removal of Mammary Cancer, with Extirpation of the Axillary Glands as a Necessary Accompaniment"), and to the Liverpool Medical Institution in the same year. They are in purport the same paper. The results of 46 cases were given. May I quote a paragraph or two from it? I said, referring to my paper written in 1877:—

"In this paper it was asserted that surgeons do not remove cancers of the breast. After an interval of five years I feel obliged to reassert the statement. Surgeons, as a rule, do not remove cancers of the breast. They persuade their patients that they do, and they almost persuade themselves. But there is always that little bit which they leave behind, and which they

fondly hope will not grow because it is such a little bit. Alas! that so little leaven should leaven the whole lump! . . . . In the present paper a principal object is to advocate the removal of the axillary glands, as well as the breast, in *all* cases, whether we can feel them enlarged or not—in fact, to make a clearing out of the axilla a necessary part of the operation for removal of the breast. I have been quietly practising this for three or four years, having been driven to the conclusion that it was the right thing to do by discovering that, even in those cases where certain glands could distinctly be felt enlarged, when the axilla was opened small ones were discovered which, although most palpably affected, were quite incapable of being affected from the outside. From this it follows that the usual fumbling in the axilla, which is commonly practised, is all nonsense. When the glands are as big as walnuts any first-year student can tell that they are affected. But what I wish with all earnestness to insist upon is that there is a stage —the early stage—when they are certainly infected, although to the touch, through layers of fat and skin, nothing amiss can be felt. When the dresser at the end of his report upon a case of mammary scirrhus, winds up with the stock phrase, 'Glands in the axilla not affected,' I immediately stop him with, 'How do you know? All that you should say is that you can't feel them, which is a totally different matter.'"

In 1887 I read a paper to the Harveian Society of London, entitled "Extirpation of the Axillary Glands a Necessary Accompaniment of Removal of the Breast for Cancer," with details of 82 cases. I there reiterated the points I have just alluded to, and added some others which had struck me:—

"Having cleared out the breast, I do not cut it adrift, but leave it suspended by its upper and outer end. I next open the axilla, and in so doing carry away a continuous mass of tissue upwards from the mamma, with the object of including in it all the lymphatic vessels going from the breast to the glands. This is an important step and should never be neglected. . . . . I have tied the axillary vein at least half a dozen times, and there was not the slightest difference afterwards between these cases and others, so that it seems certain that careful ligature thereof is not in the least a dangerous proceeding. Is it necessary to divide the pectoralis major? Very seldom, I think. I imagine that,

out of my 82 recorded cases, it was not done more than two or three times. If the floor of the axilla has been widely opened, and if the arm is properly drawn up, you can feel with your finger quite well up to the clavicle, and pinch or pull down glands from the very top of the cavity. Dividing the pectoral muscle adds somewhat to the tediousness of the operation, but that is all."

Seeing that my paper, although urging the employment of a much more extensive operation than that usually performed, did nevertheless seem to prove that greater immunity from recurrent disease was obtained thereby, I had hoped to have received some little encouragement from the London surgeons present at the meeting. Exactly the reverse was the case. In the 'International Journal of the Medical Sciences' (1888) is a most important paper by Mr. Samuel Cross, in which he said:—"In the remarks on Banks's paper at the Harveian Society in the spring of 1887, in which that surgeon reported 82 cases of extirpation of the breast along with the axillary contents, Bryant and Butlin questioned whether the complete operation afforded better results than the incomplete; and Butlin, Pick, and Cripps held that clearing out the axilla in every case was neither necessary nor judicious, although Pick asserted that the skin should be freely removed. Owen was the only member who gave cordial support to the procedure urged by Banks, the prevailing opinion being that the operation was attended with an excessive mortality, and did not lessen the risk of recurrence." Mr. Thomas Smith, by the way, thought that the patients lived longest who had never been operated upon, and that it was a question for consideration whether a local excretion of cancer did not render patients less liable to constitutional disease. I returned to Liverpool very much downcast after this meeting, but I am glad to think that after 13 years Mr. Edmund Owen and I are here to-night to rejoice in the complete triumph of the principles for which we contended on the night of March 3rd, 1887.

Mr. Butlin's Views.

"Before I enter on the discussion of this question let me say "that no personal feeling exists between myself and those who "hold the doctrine of extensive operation, and that, if I speak at

"any time warmly on the points under discussion, it is because "the subject is one in which I take the warmest interest, and on "which I hold very decided opinions. I entertain a great respect "for both Dr. Gross and Mr. Banks, and only mention their "names as the distinguished leaders of a school of surgery from "which I venture to disagree." Mr. Butlin was good enough to make these very kind remarks in 1887, and I would like here entirely to reciprocate them. After this very generous introduction, however, he made some criticisms, which I am sure he will forgive me reproducing here, along with some remarks made in 1881 and 1898.

1881. *From the 'Transactions of the International Medical Congress.'*—Mr. Butlin said that he quite agreed with Dr. Gross that in the case of any tumour of suspicious character the sooner it was removed and the more freely it was removed the greater were the prospects of complete recovery; and, further, he agreed thoroughly with Dr. Gross that in the case of carcinomatous tumours it was highly desirable to extend the operation so as to examine the axilla if there were any doubtful fulness, even though no definite enlargement of the glands could be detected.

1887. *From the 'Operative Surgery of Malignant Disease.'*—Mr. Butlin wrote that no prolonged search need be made for the lymphatic vessels lying between the breast and the axillary glands. If there were not any distinct cords to be felt running up into the axilla, it was not necessary to search for the lines of lymphatic vessels or to remove a large quantity of the fat and other tissues in order to ensure their removal. Referring to Gross and Banks, these surgeons were in the habit of cutting off the mamma with all the skin which covers it, dissecting off the fascia from the pectoral muscle, searching for outlying lobules of the gland in the surrounding tissues, carrying the incision into the axilla, whether enlarged glands were previously felt there or not, and clearing out the glands and all easily removable structures. Both surgeons were impressed with the absolute necessity of very extensive operations if the patient was to be afforded a reasonable chance of complete recovery from the disease. Both surgeons had pushed this teaching so far that it had already gained many adherents, although he believed rather among foreign than among English surgeons. Indeed,

he was surprised to find how strong an opinion was expressed against it by some of the English operating surgeons at a recent meeting of the Harveian Society in the discussion of a paper by Mr. Banks. As he entirely disapproved of this wholesale way of treating cancers of the breast, and believed it to be unscientific and needlessly cruel to many women, he would venture to criticise it as thoroughly as he could. The operation recommended by Banks was not such an operation as that which was frequently practised in London and provincial hospitals, but was much more sweeping, for it included the whole of the skin over the breast, and not merely over the tumour. In fact the breast was, so to speak, lopped off as if the operation had been performed with a sword or a carving knife. Now, he quite admitted that, if the entire breast was to be removed in every instance of cancer, the operation should be of this sweeping character. But he was strongly of opinion that the practice of removing the whole of the breast in every case was theoretically and practically wrong. Probably a long time and much false reasoning were required to induce the acceptance of the doctrine of the necessity of the removal of the mamma for cancers of small size limited to a part of it, and he was afraid a still longer time and much good reasoning would be required to convince surgeons that the practice was radically wrong. So far as the removal of the glands was concerned, by all means let them be removed when they can be felt enlarged and within reach of removal, and in cases in which there was a doubtful fulness which was not apparent on the other side. But let no theoretical considerations lead them further than this, and on no account let the axilla be opened in search of diseased glands of the existence of which there was not the least outward or palpable sign. In reply to all the arguments he had brought forward, Mr. Banks had at his command a very strong answer—namely, his results. He quite admitted—and with very great pleasure admitted—that, so far as his later results were concerned, they were the best he had ever seen from operations performed with the knife. The superiority of these statistics may be accounted for, not by the fact that the entire breast was removed in every instance, but because Mr. Banks was in the habit of sweeping away the disease itself and the surrounding textures in a fashion at which most operators would be horrified.

1898.—In the number of the 'British Medical Journal' for November 26th, 1898, Mr. Butlin wrote that he had read Professor Halsted's paper on the "Results of Operations for the Cure of Cancer of the Breast" (1894), and his results appeared to him to justify a thorough reconsideration of the subject of the operative surgery of cancer of the breast. This operation was suggested by a paper in 'Langenbeck's Archiv' (1889) on the "Causes of Local Recurrence of Cancer after Amputation," by Dr. Heidenhain, and he had studied it along with a paper on the "Surgical Anatomy of the Breast and Lymphatic Glands," reprinted from the 'Edinburgh Medical Journal' (1892), which the author, Mr. Harold Stiles, had been so kind as to send to him. Both authors had come to the conclusion, from a general and microscopical examination of a number of breasts which had been removed, that cancer of the breast rapidly extends back towards and then into the pectoral fascia, even when there appears to the naked eye to be a considerable interval between the posterior border of the tumour, and that the pectoral fascia is often very difficult to separate satisfactorily from the surface of the muscle. Looking back on his own clinical experience, in order to see how far it seemed to be consistent with the investigations of Heidenhain and Stiles, he could quickly remember several cases in which recurrence had taken place in the substance of the pectoral muscle, after the removal of a cancer which appeared to be quite separate from the pectoral fascia, and even after the fascia had been roughly dissected off. (Then followed a long description of Halsted's operation, with Mr. Butlin's expressions of belief in its value.)

Now, if any gentleman will take the trouble to turn up Mr. Butlin's work on 'The Operative Surgery of Malignant Disease,' he will find that the last chapter is devoted very largely to three things: (1) A subdued admiration of a certain Dr. Bougard, of Brussels, who used caustic paste for the destruction of cancerous mammæ as well as of the axillary glands down to the vicinity of the main vessels and nerves; (2) an advocacy of the mere removal of malignant tumours of the breast and nothing more, seeing that the evidence for the removal of the entire breast was only very partially true; and (3) a very sharp attack upon the doctrines then being promulgated by Dr. Gross

and myself. Dr. Gross presently wrote these words:—"Had so good a pathologist as Butlin been in possession of the facts derived from my practice and that of Kuester, he would scarcely have given utterance to so pernicious a doctrine—a doctrine which, we fear, will so influence English surgeons as to counteract in great measure the recent advances that have been made in the treatment of carcinoma of the breast."

For myself I offer no criticism. I have merely quoted the *ipsissima verba* of Mr. Butlin in 1881, 1887, and 1898, and leave them to the consideration of the surgical profession.

Halsted's and other Operations.

In the 'British Medical Journal' of December 8th, 1898, is a very full description of Halsted's operation. It contains 15 paragraphs, with very minute details, down to the grasping of the tumour with the left hand, and the scrupulous dissection of the axilla with "the sharpest possible knife." The only points of any fundamental importance in which it differs from the operation ascribed by Mr. Butlin to myself (just quoted) consist in the removal of the sternal portion of the pectoralis major and the division of the pectoralis minor with reflection of that muscle. This is described as necessary for the complete removal of the glands in the axilla. I desire entirely to dissent from this view. I do not believe it necessary, still less that the entire pectoralis major need be removed, as is now Dr. Halsted's present contention. If the arm be properly manipulated and the great pectoral wall dragged upwards and inwards, the very topmost point of the axilla can be cleaned out. Moreover, if there is any doubt at all, the great pectoral muscle can be cut across with very little trouble, and then stitched together with catgut sutures, as I have every now and then done. In the 'Transactions of the American Surgical Association' Dr. Halsted relates that he now almost invariably cleans out the supraclavicular region, and that our rule ought to be, "Operate on the neck in every case." For this purpose, however, he does not divide the clavicle as he did five or six years ago; but if it were very desirable to remove the supraclavicular contents in one piece with the axillary contents and breast, one need not hesitate to excise, if necessary, even the entire clavicle. He considers the neck involvement is no more

serious than the axillary, and that the neck can be cleaned out just as thoroughly as the axillary region. He believes that in the near future we shall remove the mediastinal contents at some of our primary operations. It seems a pity he did not throw in the liver and lungs when he was at it.

With every wish to defer to the opinions of those from whom I differ, I feel sure that they will permit me to state my views as plainly as many of them have stated theirs. Some little apology I may plead, namely, that I have removed the breast and cleaned out the axilla over 250 times. It is a locality with which I am necessarily fairly familiar. Well, I have stated that I do not consider that carrying away the entire great pectoral muscle and dividing the small one are at all necessary for the satisfactory removal of the axillary contents. Moreover, I have given up attempting to get any good out of clearing out the supraclavicular region. In former days, when I thought I could remove any cancer, I did a good many cases, but never one survived. I do not say I will not do it again if an apparently suitable case occurs. I did it 12 months ago on a lady who had a small cancer situated about an inch below the clavicle, which had directly affected the cervical glands, but it was a failure as usual. Dr. Coley, in the article "Cancer," in 'Twentieth Century Practice of Medicine,' says:—"The only positive evidence Halsted's statistics afford us as to the value of this operation of cleaning out the supraclavicular fossa rests upon two cases in which the operation was done secondarily, and in which the patients remained well three and three and a half years after the operation. In view of what I have already said regarding the failure of a three-year limit as establishing a cure of the disease, we cannot consider the question as settled without much stronger evidence." *Natürlich!* My notion is this, that when cancer has laid hold of the supraclavicular glands it has got such a grip as to be past extirpation. It is like a burning house. When the fire is beginning, even some buckets of water will put it out; when it has got to a certain pitch, a whole fire brigade will not quench it. Moreover, an experienced fireman will tell you the minute he sees the fire whether he can put it out or not. Bennett May, in his 'Ingleby Lectures' for 1897, says that he thinks it very doubtful whether some of these modern extensive operative procedures confer any additional immunity from recurrence at all commensurate with

their severity. It is clear that in both classes of operation the majority of fatal cases die of internal metastasis without external recurrence, and against these no operation can protect. Dr. Matas, of New Orleans, at a meeting of the American Surgical Association in 1898, said, speaking of Halsted and Meyer's operations, that he firmly believed that surgery had there nearly reached, if it had not already attained, its maximum expression of effectiveness, beyond which it is absolutely impossible to advance without great risk to life, or with any further expectation of increasing the chances of permanent recovery. I entirely agree with Mr. Marmaduke Sheild when he says that the swing of the pendulum of surgical opinion is generally too extreme, and has now turned from too slight to too extensive operating.

One word more in reference to removing the great pectoral muscle. Do cancerous recurrences take place in the substance of that muscle? Are they found in the interval between the great and small pectoral muscles? Taking the latter query first, I have never seen such an occurrence. Referring to the first, I believe that when the great pectoral muscle is the site of recurrent growths they creep into it from the subcutaneous tissues and pectoral fascia. They do not primarily originate in the muscle. There is, therefore, no need for its removal on that ground. No man in this country has had more experience of the pathology of cancer than Mr. Plimmer, from his connection as pathologist to the Cancer Hospital. In a conversation with him the other day he perfectly concurred in this view, and, moreover, stated that he had never known anybody live long after distinct invasion of the supraclavicular glands.

I think I need say very little concerning the operation of Berger, which means removing the upper limb along with the scapula. It may be dismissed with the remark I once heard made by my friend Mr. Owen about another matter, slightly altered from the French marshal's observation about Balaclava: "C'est magnifique mais ce n'est pas la chirurgerie." Surely there are hypodermic syringes and morphine still in the land!

In the operations, the statistics of which I am about to present to you, my plan has been this:—I never make an angular, triangular, or multiangular incision, for they are a farce. I draw a circular "ring fence" with a knife outside the apparent limits of the mamma if the tumour is central, and, if it is lateral, the

"ring fence" should bulge well beyond the tumour. I then slip the blade on the flat beneath the skin for about 2 inches, and undercut it all round. This skin is then well pulled back at successive points by an assistant, and where the undercutting stops, there the removal begins. So one gets a long way beyond the rough limits of the mamma proper. Then the tumour is removed, and the fascia dissected off the great pectoral muscle. The great advantage of the undercutting is that one can get the fascia off the muscle as far inwards as the sternum, down to the ribs, and up almost to the clavicle. As Mr. Butlin mentions, the removal of the fascia was one of the points dwelt upon by Dr. Gross and myself very many years ago. The axilla should then be opened by an incision just under the edge of the great pectoral and the upper end of the gland, and the whole contents of this cavity, as far as they can be held together, carried away in one mass. The importance of this lies in the fact that recurrence in the axilla takes place, for the most part, not in glands that are left behind, but in fragments of lymphatic vessels. I feel convinced of this. A good plan is to get hold soon of the axillary vein, and then there is nothing else to trouble about. One must take the matter very patiently, getting everything out of the cavity that can be got, and cleaning up the digitations of the serratus magnus. On no account do I introduce the "sharpest possible knife" into the armpit, nor any knife at all. I use a dissecting tool, two very long, strong pairs of dissecting forceps, and curved, probe-pointed scissors. But everybody according to his taste, except beginners, who ought not to use knives here.

A drain-hole should now be made just above the edge of the latissimus dorsi. Concerning the covering in of the great wound, it is remarkable what can be done towards pulling it together by very strong wire and by the help of lead buttons. The very top of the axillary incision should be kept open. I put a little bit of gauze into this aperture and into the drain-hole below. I wash my hands well with hot water and soap before operating. I pour hot water constantly over the wound during the operation, and see that it is dry from blood at the end. I use no disinfectant fluids, and I dress the wound with a piece of perchloride gauze and a pad of absorbent wood wool over it. I have used this plan of operating for over 20 years, and have only altered it slightly in the following points:—After reading my paper to the Harveian

Society in 1887, so soon as I had recovered from my first discouragement, I proceeded to remove more skin than ever, for I became more and more convinced that skin is the really dangerous tissue in this matter of cancer of the breast. Thiersch grafting was not then invented, and so I had great difficulty in covering the big wounds. It was then that I began the practice of undercutting, which I have demonstrated and taught ever since. If a wound of great size is left uncovered nowadays, we have the Thiersching to fall back upon. I have used it both at the time of operation, and afterwards when granulation was going on, and I prefer the latter plan.

A view is now being taught that there is great danger of infecting the wound with the knife that has made the exploratory incision through the tumours. I have seen no evidence whatever to this effect, and the extreme difficulty of getting the strongest cancer juice to reproduce anything in any cultivation medium is not in favour of it. If the operator is afraid of such a contingency let him wash his knife.

Before leaving this subject I wish to make a few remarks, and I find great difficulty in making them, so much do I fear lest what I may say should be regarded in any way as derogatory to the talents of two gentlemen for whom I have a very great admiration —Dr. Heidenhain and Mr. Stiles. In 1887 Mr. Butlin said with reference to mammary operations:—"It was only during the "writing of this book that I became impressed with the necessity "for reconsideration of the subject." In 1898, in consequence of having read Dr. Halsted's paper, the results "appeared to me to "justify a thorough reconsideration of the surgery of cancer of the "breast." In this he was fortified by the work of Heidenhain and Stiles. Now, the researches of these gentlemen on the minute lymphatics of the breast and neighbouring tissues and the way cancer creeps into them are of the greatest interest from an anatomical point of view. But I do not see that they have done anything very specially to influence the views of men who are occupied practically in endeavouring to eradicate mammary cancer. Dr. Coley says:—"Still later, in 1889, Heidenhain, by "his very careful and scientific investigation as to the manner in "which cancer cells extended from the original focus of the "disease into the neighbouring parts, demonstrated the facts, "many of which Moore and Gross had already from clinical

"observations previously assumed as true. The gist of Heiden-
"hain's results may be summed up in a few words. A cancerous
"tumour that has become sufficiently large to be recognised
"clinically has already scattered its infective cells throughout
"the entire breast, the neighbouring chain of glands and in
"many instances the fascia overlying the pectoral muscle."
Quite so, but there was no surgeon in 1889 who did know this as a matter of fact, although it had not actually been demonstrated by the microscope. It was not that surgeons did not know this—it was that they were not bold enough to act on their knowledge. In justice to myself, and to the memory of my late friend Gross, I think it rather hard that a certain group of surgeons should date the origin of the modern operative treatment of cancer from the microscopic researches of Heidenhain and Stiles, and the labours of Halsted and themselves.

TABLE OF STATISTICS.

Series 1.—*Cases in which the Disease proved Fatal by Local Recurrence.*

(*a*) Cases which lived between 1 and 3 years.

Name.	Age.	Duration of Life.		Name.	Age.	Duration of Life.	
		Y.	M.			Y.	M.
1 Mary N. ..	(?)	0	5	26 Mrs. C. ..	40	1	4
2 Miss B. ..	50	0	6	27 Mary W. ..	45	1	6
3 Eliz. P. ..	50	0	6	28 Eliz. G. ..	40	1	4
4 Rachel K. ..	61	0	6	29 Jane H. ..	48	1	6
5 Mrs. C... ..	50	0	8	30 Mrs. A. ..	52	1	6
6 Miss S... ..	46	0	9	31 Mrs. B. ..	(?)	1	8
7 Eliz. M. ..	47	0	9	32 Janet J. ..	46	1	8
8 Miss B... ..	54	0	10	33 Margaret O...	46	1	8
9 Mrs. H. ..	35	0	10	34 Ann O. ..	44	1	9
10 Mrs. V. W. ..	(?)	0	10	35 Jane C. ..	56	2	0
11 Han. R. B. ..	41	0	11	36 Mary A. M...	45	2	0
12 Mary A. K. ..	33	1	0	37 Annie T. ..	44	2	0
13 Anna Mc. ..	39	1	0	38 Jane R. ..	39	2	0
14 Eliz. C. C. ..	41	1	0	39 Margaret J...	53	2	0
15 Mrs. K. ..	(?)	1	0	40 Eliz. P. ..	52	2	0
16 Eliz. D. ..	58	1	0	41 Miss W. ..	56	2	0
17 E. H.	67	1	0	42 Miss L. ..	36	2	2
18 Julia K. ..	46	1	1	43 Mrs. F. ..	42	2	1
19 Mrs. L'E. ..	60	1	1	44 Ann W. ..	54	2	3
20 Miss M. ..	41	1	1	45 Jane H. ..	49	2	4
21 Amy H. ..	35	1	2	46 Jane H. ..	38	2	6
22 Mrs. G. ..	45	1	3	47 Ann W. ..	54	2	6
23 Miss B. ..	55	1	2	48 Miss S. ..	40	2	6
24 Mrs. H. ..	55	1	2	49 Mrs. M. ..	50	2	6
25 Ann H. ..	42	1	4				

(*b*) Cases which lived between 3 and 14 years.

Name.	Age.	Duration of Life.		Name.	Age.	Duration of Life.	
		Y.	M.			Y.	M.
50 Eliz. R. ..	40	3	0	59 Marg. W. ..	46	4	2
51 Cath. H. ..	52	3	2	60 Mrs. F. ..	57	4	5
52 Mrs. F... ..	43	3	3	61 Jane B. ..	50	4	6
53 Eliz. C... ..	48	3	5	62 Mrs. T. ..	48	5	0
54 Jane T... ..	50	3	6	63 Mrs. T. ..	34	5	7
55 Mary C. ..	48	3	3	64 Mary A. ..	67	5	7
56 Mrs. W. ..	(?)	4	0	65 Mary M. ..	36	7	3
57 Mrs. M. J. ..	43	4	0	66 Mary S. ..	50	12	0
58 Mary J. T. ..	47	4	0	67 Mary D. ..	40	14	0

Series 2.—Cases Alive and Remaining Free from Disease.

(*a*) From 1 to 3 years.

Name.	Age.	Duration of Life.		Name.	Age.	Duration of Life.	
		Y.	M.			Y.	M.
1 Mrs. C. M. ..	40	1	5	6 Mrs. S. ..	56	2	0
2 Margt. B. ..	49	1	6	7 Amelia H. ..	58	2	4
3 Mrs. T... ..	39	1	8	8 Jane E. ..	57	2	5
4 Miss T... ..	(?)	1	10	9 Annie R. ..	53	2	5
5 Miss R... ..	54	1	10	10 Miss C. ..	56	2	6

(*b*) From 3 to 21 years.

11 Mrs. E... ..	56	3	0	30 Margt. B. ..	50	7	2
12 Miss S... ..	45	3	0	31 Louisa S. ..	55	7	6
13 Miss O... ..	24	3	0	32 Mrs. B. ..	51	8	0
14 Martha H. ..	46	3	0	33 Margt. C. ..	(?)	8	0
15 Miss A. J. ..	45	3	1	34 Margt. S. ..	63	8	2
16 Annie V. ..	42	3	3	35 Sarah C. ..	64	8	5
17 Mary M. ..	38	3	6	36 Miss H. ..	39	8	6
18 Mrs. S... ..	34	3	5	37 Eliza J. ..	69	10	0
19 Mrs. B... ..	50	4	4	38 Miss R. ..	62	10	0
20 Mrs. L... ..	50	4	10	39 Miss C. ..	40	10	3
21 Mrs. L... ..	62	4	3	40 Mrs. T. ..	55	11	0
22 Mrs. W. ..	54	5	0	41 Margt. F. ..	41	12	7
23 Cthrn. S. ..	31	5	5	42 Mrs. B. ..	46	14	0
24 Mrs. G. ..	50	5	6	43 Miss R. ..	52	14	3
25 Kate B. ..	52	6	0	44 C. R... ..	53	14	6
26 Mrs. C... ..	45	6	4	45 Mrs. R. ..	50	14	10
27 H. W.	68	5	4	46 Miss M. ..	44	16	2
28 Mrs. F... ..	47	7	0	47 Mrs. M. ..	42	17	10
29 Louisa B. ..	43	7	0	48 Maria R. ..	49	21	0

Series 3.—Cases where the Patient Died without Local or General Return as far as Known.

	Name.	Age.	Duration of Life.		Causes of Death.
			Y.	M.	
1	Martha K.	48	0	2	Died in asylum—mania (?).
2	Mary W.	56	1	4	Acute pleurisy.
3	Alice Ann D. ..	37	1	0	Heart disease.
4	Margaret J. ..	36	1	0	Childbirth.
5	Mrs. L.	43	1	0	Acute meningitis.
6	Ellen W.	41	1	6	Some nerve disease.
7	Mrs. A. B.	49	1	8	Acute pneumonia.
8	Mrs. F.	45	1	10	Sudden coma and collapse.
9	Margaret B. ..	62	1	9	(?)
10	Marjory G. ..	32	2	0	(?)
11	Margaret C. ..	46	2	6	Bronchitis and heart disease.
12	Mrs. C.	57	2	11	Pneumonia.
13	Mrs. B.	(?)	2	6	Acute tuberculous phthisis (doctor's report).
14	Anna C.	56	2	8	Liver cirrhosis.
15	Mrs. A.	48	2	7	Phthisis and hæmoptysis.
16	Elizabeth M. ..	54	3	0	(?)
17	Mrs. W.	38	3	8	Diabetes.
18	Barbara McG. ..	50	3	9	Acute bronchitis.
19	Mary J.	(?)	3	8	Do.
20	Eliza M.	42	4	0	Some cerebral disease.
21	Margaret H. ..	63	4	0	Renal and general debility.
22	Miss C.	61	5	6	"No secondary cancer" (doctor's report).
23	Elizabeth W. ..	55	5	6	M. (?)
24	Mary L.	43	6	0	Pneumonia.
25	Miss A.	(?)	8	0	Apoplexy.
26	Ann J.	49	8	2	(?)
27	Mrs. L.	56	9	0	Heart disease.
28	Agnes R. M. ..	48	9	3	Apoplexy.
29	Miss T.	50	16	0	Influenza.
30	Mrs. W.	53	18	0	"Weak heart" at 71.

Series 4.—Cases with no Local but General Recurrence.

	Name.	Age.	Duration of Life.		Sites and Causes of Recurrence.
			Y.	M.	
1	Mary B.	49	0	3	Liver.
2	Jane S.	26	0	8	Lung.
3	Mrs. H.	42	1	0	Cancerous pleurisy.
4	Caroline O'B. ..	50	1	2	Pelvic organs.
5	Jane C.	42	1	2	Humerus.
6	Edith T.	40	1	4	Liver.
7	Mrs. G.	53	1	6	Lung.
8	Annie H.	42	2	0	Stomach—probably cancer.
9	Mrs. L.	61	2	1	Spinal column.
10	Sarah P.	35	2	6	Lung.
11	Mrs. E.	40	3	0	Lung—probably cancer.
12	Mrs. B.	49	3	0	Stomach.
13	Jane C.	51	3	1	Rectum.
14	Mrs. B.	(?)	3	10	Uterus.
15	Miss B.	56	3	10	Lung.
16	Elizabeth J. ..	(?)	4	0	Uterus and rectum.
17	Barbara G... ..	40	4	0	Lung—probably cancer.
18	Mrs. E.	(?)	4	0	Lung.
19	Mrs. B.	33	4	0	Rectum.
20	Mrs. J.	62	4	10	Cæcum.
21	Mrs. J.	52	5	0	Rectum.
22	Mrs. R.	52	5	0	Œsophageal disease (?).
23	Margaret C. ..	(?)	5	6	"Relapse."
24	Miss R.	48	5	2	Lung.
25	Mary W.	67	7	0	Lung.
26	Mrs. W.	49	7	2	Stomach and pleura.
27	Mrs. F.	(?)	7	4	Liver and rectum.
28	Mrs. E.	46	10	6	Ovary.
29	Mrs. H.	58	10	6	Liver.
30	Mrs. Anne J. ..	38	14	0	Stomach—probably cancer.

Series 5.—Imperfect Cases in which it was found Impossible to Remove the whole Disease at the Time of Operation.

	Name.	Age.	Duration of Life.			Name.	Age.	Duration of Life.	
			Y.	M.				Y.	M.
1	Esther G. ..	52	0	6	5	Sarah R. ..	51	1	6
2	Ann H... ..	36	0	7	6	Fanny P. ..	49	0	4
3	M. A. C. ..	51	0	7	7	Ann D. ..	54	2	0
4	Martha L. ..	42	1	1					

Series 6.—Cases done under One Year and about which nothing can as yet be said.

Sixteen cases have been operated upon during the past year successfully.

Series 7.—Cases which Recovered from Operation but Cannot be Traced.

These numbered 15, of whom one was alive after 2 years 10 months, another after 2 years 6 months, and a third after 8 months.

Series 8.—Cases Operated upon during 1896, 1897, 1898, 1899, *and Three Months of* 1900.

1896.

1	Miss S.	March.
2	Mrs. E.	January.
3	Mrs. S.	November.
4	Mrs. H.	October.
5	Mrs. R.	November.
6	E. G.	August.
7	B. W.	April.
8	J. McG.	February.
9	M. H.	October.
10	C. Ob.	August.
11	M. M.	October.
12	A. V.	December.
13	J. C.	March.
14	J. K.	December.

1897.

15	Miss B.	April.
16	Miss C.	December.
17	Miss E.	March.
18	Miss S.	April.
19	Miss D.	May.
20	A. R.	October.
21	J. K.	January.
22	A. V.	February.
23	J. L.	November.
24	A. H.	September.
25	E. T.	November.
26	M. W.	January.
27	M. H.	May.
28	A. J.	March.
29	A. W.	June.
30	A. H.	November.

1898.

31	Mrs. T.	June.
32	Miss W. ..	November.
33	M. N.	November.
34	Miss T.	June.
35	Miss R.	June.
36	Miss S.	March.
37	A. McC.	April.
38	M. B.	September.
39	C. M.	October.
40	J. K.	December.
41	H. W.	March.

1899.

42	Miss C.	December.
43	Mrs. D.	April.
44	Mrs. D.	March.
45	Mrs. E.	October.
46	Mrs. C.	March.
47	Mrs. H.	January.
48	Mrs. F.	October.
49	Mrs. B.	January.
50	Mrs. H.	November.
51	R. K.	August.
52	M. S.	April.
53	M. B.	April.
54	A. H.	June.
55	M. D.	July.
56	E. C.	June.
57	A. McN.	April.

1900.

58	Mrs. L.	March.
59	Mrs. P.	March.
60	Mrs. F.	January.

Among these 60 cases there was no fatal one.

The above table of statistics speaks for itself. I am sorry that during the removal from our old Liverpool Infirmary to the present one the histories of many cases were lost. But those I have laid before you have been collected with as much care and accuracy as I could give to the matter. Mistakes there may be, but I am certain that they cannot be sufficiently numerous to invalidate in any way the general conclusions that may be drawn from the table. The cases number 213, of which 175 are available for statistical comparison. In Series 1 it is noteworthy that of the 67 persons who died of local recurrence 17 lived for periods between 3 and 13 years. Series 2 shows 38 persons alive and well between 3 and 21 years after operation. To Series 3 it may be objected that I cannot prove that some of the patients did not die from internal recurrence. It is a fair argument, and all I can reply is that I have taken all the pains I could to make sure of the causes of death, and have put any doubtful ones into Series 4. But, after all, if a breast cancer seems a fit subject for removal we do not speculate on internal recurrence, for our business as surgeons is to find out whether we can eradicate the disease locally, so that we may be able to give patients who apply early for relief a good prospect of future immunity. Now, the first four series comprise 175 patients, and of these 108 have remained free from local recurrence. Of the 108 there have lived 73 over three years as follows:—

Cases that lived between 3 and 6 years after operation	40
Cases that lived between 7 and 14 years after operation	28
Cases that lived between 16 and 21 years after operation	5

Take from these 73 cases half-a-dozen in case of error, and still it follows that for every one of my 67 failures I can put against it a case where the patient remained free from 3 to 21 years. With regard to tables of statistics in general, the plan of taking percentages of small numbers of cases ought to be abolished. The figures in a surgical article nowadays make it look like an extract from an accountant's report on a bankruptcy case. As for the three years' cure system, it, too, must be abolished if people are to be instructed fairly. Let no woman be reckoned cured of cancer of the breast till she is dead.

And now, gentlemen, I have only to thank you for the patient courtesy with which you have listened to my poor attempts in a line of surgery to which I have devoted the best efforts of my life during the past quarter of a century.

Explanation of the Illustrations—Parasites in Mammary Carcinoma.

Fig. 7.—Parasites in a mammary carcinoma; two in centre well defined. × 375. Reichert $\frac{1}{15}$ oil immersion. Dr. R. J. M. Buchanan.

Fig. 8.—Parasites in an axillary gland secondary to mammary carcinoma. × 375. Reichert $\frac{1}{15}$ oil immersion. Dr. R. J. M. Buchanan.

Fig. 9.—A single well-defined parasite from infiltrated skin over a mammary carcinoma. × 375. Reichert $\frac{1}{15}$ oil immersion. Dr. R. J. M. Buchanan.

Fig. 10.—Group of parasites in a mammary carcinoma. × 350. Mr. Keith W. Monsarrat.

Fig. 11.—Coverslip preparation of parasites cultivated on artificial media from a mammary carcinoma. × 75. Low power. Mr. Keith W. Monsarrat.

Fig. 12.—Lung of guinea-pig showing parasites the result of the inoculation of cultivation, Fig. 11. × 350. Mr. Keith W. Monsarrat.

The specimens were prepared by Dr. R. J. M. Buchanan and Mr. Keith W. Monsarrat.

FIG. 7.

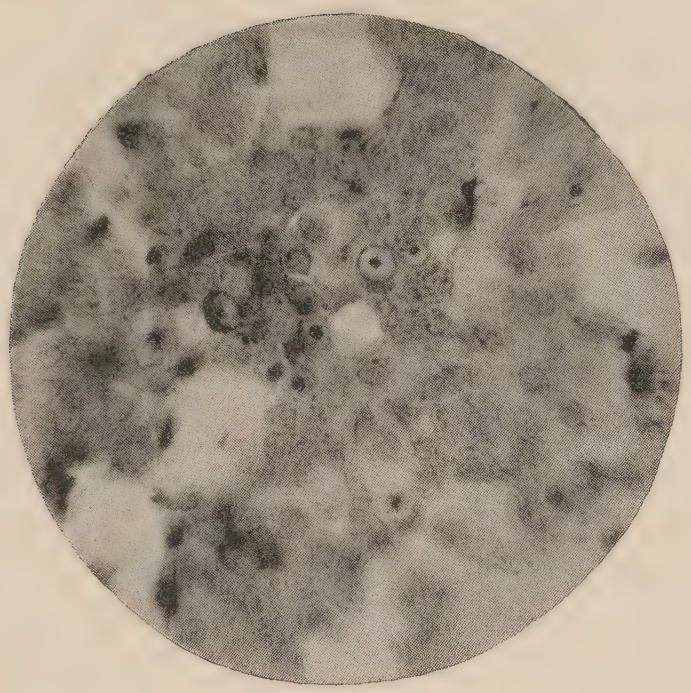


FIG. 8.

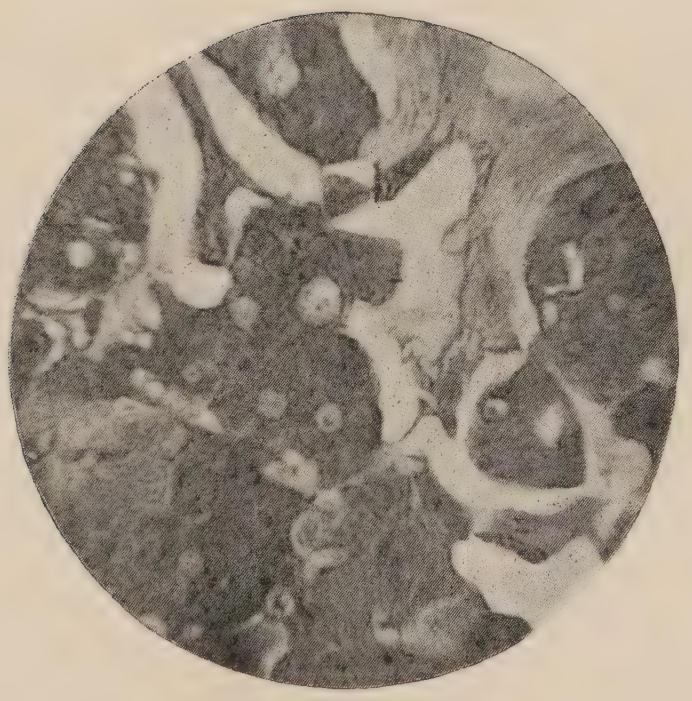


Fig. 9.

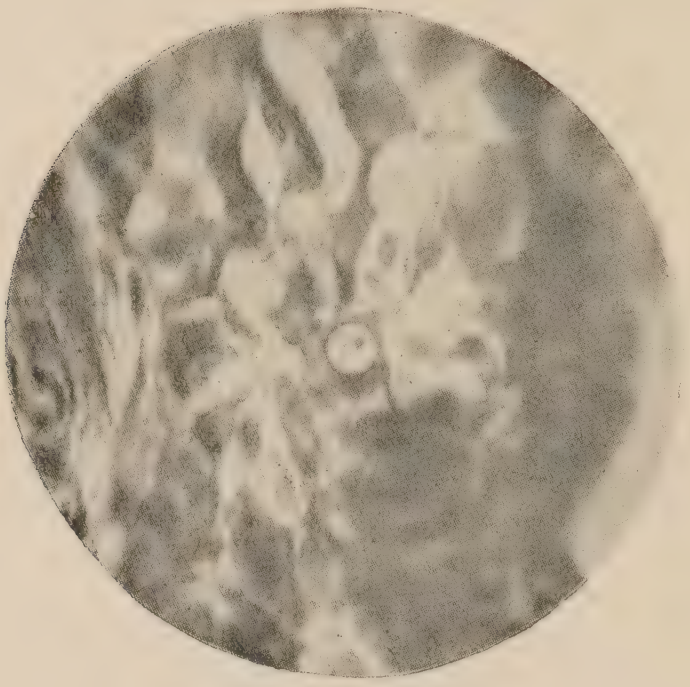


Fig. 10.

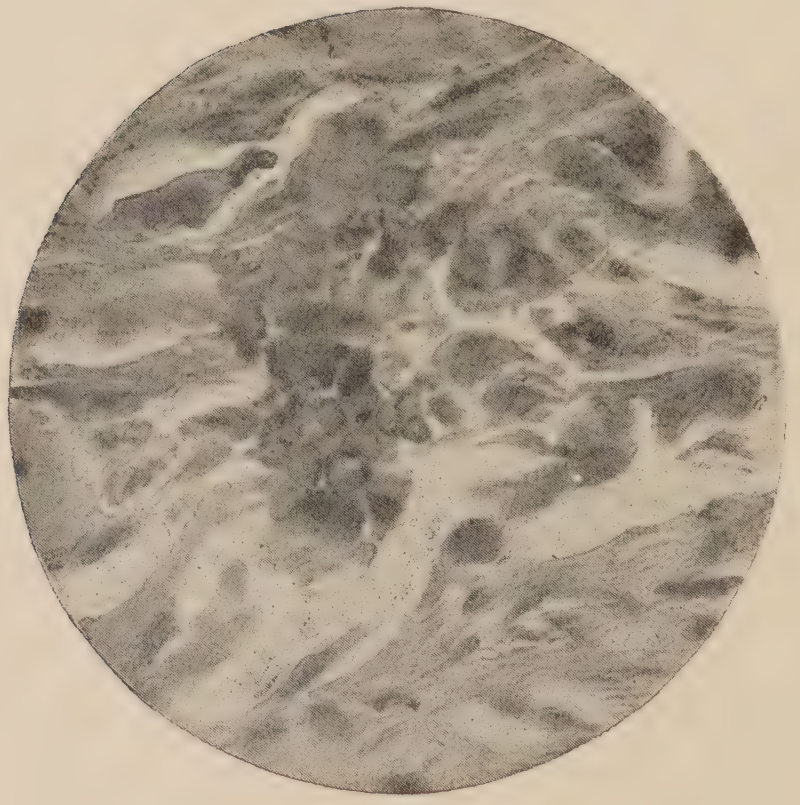
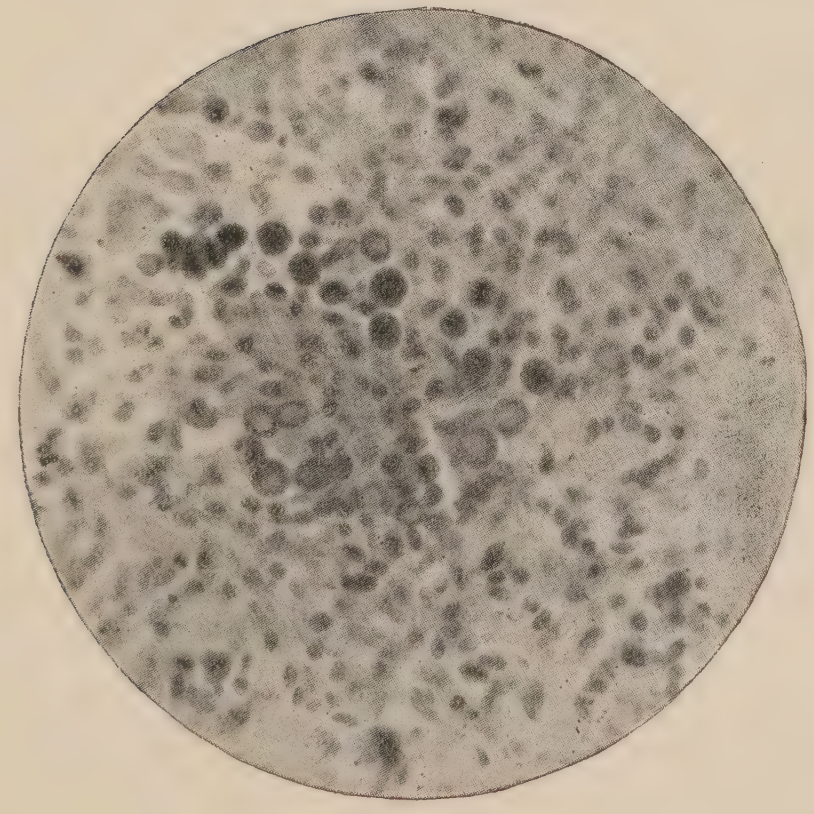


FIG. 11.

FIG. 12.



March 12*th*, 1900.

A NEW METHOD OF PERFORMING PERINEAL PROSTATECTOMY.

By P. J. Freyer, M.A., M.D., M.Ch.

The vast majority of cases of senile enlargement of the prostate are, in my opinion, best treated by careful, cleanly, and judicious catheterism. There is, however, a minority in which a surgical operation of some kind is advisable, and a still smaller proportion in which an operation is imperative to save the patient's life or ameliorate his sufferings. I propose this evening introducing to your notice, by means of an illustrative case from practice, a new method of removing certain forms of enlarged prostate through the perineum. In order to clear the way for your appreciation of this procedure, I shall classify briefly the various recognised surgical operations for the disease under consideration as follows :—

1. *Palliative operations*, undertaken when there is cystitis or some other complication present, rendering catheterism impossible or extremely difficult—

(i) Suprapubic cystotomy, with temporary or permanent drainage through a retained tube.
(ii) Perineal urethrotomy, with temporary or permanent drainage.

2. *Operations undertaken for the purpose of inducing permanent atrophy or shrinkage of the prostate*—

(i) Castration.
(ii) Vasectomy.

3. *Radical operations*, which aim at removing a part or the whole of the obstructing portion of the gland—

(i) Division of a median obstruction by means of the galvano-cautery introduced through the urethra (Bottini) or through a perineal urethrotomy (Wishard).
(ii) Removal of a median growth through a perineal opening in the urethra by cutting forceps or other instrument.

(iii) Suprapubic prostatectomy (McGill), suitable only for enlarged middle lobe, ring of hypertrophied tissue round the orifice of the urethra, or enlargements of the lateral lobes projecting into the bladder.

(iv) Perineal prostatectomy by Dittel's method, which consists in removing a wedge-shaped portion from the under surface of one or both lateral lobes, through an incision extending from the median raphe round the sphincter ani to the tip of the coccyx, the urethra and bladder being left intact.

(v) Nicoll's operation, which is similar to this last, only that a preliminary suprapubic cystotomy is performed, for the purpose of introducing a finger into the bladder, with a view to pushing the prostatic tumour into the perineal wound, thus facilitating its removal.

In the operation that I am about to describe advantage is taken of the perineal incision recommended by Dittel, but Nicoll's object is attained by a much less heroic and dangerous proceeding than suprapubic cystotomy—namely, by a preliminary perineal opening into the urethra :—

A gentleman, aged 59, sent by Dr. Richard Heath, of St. Leonards-on-Sea, consulted me on October 11th, 1889, suffering from the following symptoms : Increased frequency of micturition, averaging about ten or eleven times by day and seven or eight times by night ; difficulty in starting the stream, necessitating much straining to effect this,—he finds that when he places his hand on the hypogastric region and pushes the abdomen upwards the stream starts more readily ; diminished force of the stream ; dribbling at the end of micturition ; sometimes the stream is intermittent. The urine is clear as a rule ; some eight months ago it was very dark in colour, but patient cannot say if it contained blood. Pain and discomfort behind the glands during micturition, and discomfort above the pubes before the act. The symptoms have existed three years, and have been gradually growing worse. At first they were more troublesome at night, but latterly they have been as bad in the daytime. They are increased by exercise, particularly in walking.

I sounded the patient, but could find no stone. There was some difficulty in passing the instrument over the enlarged prostate. Residual urine, $4\frac{1}{2}$ ounces ; trace of albumen. Per rectum there was a tumour felt in the right side of the prostate, twice the size of a walnut, very dense throughout, with a nodule of intense hardness, which was very painful on pressure. This nodule felt like a stone embedded in the prostate. The left lobe of the prostate was scarcely enlarged and quite soft.

On the 17th I made a cystoscopic examination, the patient being anæsthetised by Dr. Dudley Buxton, and Mr. Victor Horsley being present in consultation. The right lobe of the prostate was seen to be

enlarged and projecting into the bladder, the size of a walnut, irregularly nodulated, and of a dark brown colour. The left lobe was not visible in the bladder, which, generally, presented a healthy appearance, except that the mucous membrane was trabeculated, the result of constant straining and backward pressure of urine. After consultation, exploration of the bladder by a perineal urethrotomy was decided on, and this was carried out next day.

The urethra was opened in front of the prostate on a staff. On passing my finger through the opening I found a round, prominent mass, involving the right lobe of the prostate, projecting into the bladder, but no stone could be detected. On inserting the forefinger of my other hand into the rectum, the hard nodule was felt as before, and the tumour clearly defined, rounded and intensely hard, the left lobe of the prostate being almost normal. After consultation with Mr. Horsley, we determined to remove the tumour perineally. A crescentic incision about 4 inches long was made from the median raphe at the lower end of the urethral wound, backwards round the anus to the coccyx on the right side. The dissection was carried deep into the ischio-rectal fossa, the rectum being drawn inwards by a broad retractor. The left forefinger was passed through the urethral wound into the bladder and hooked round the tumour, which was pressed out into the wound, and thus came well into view. The capsule of the prostate was incised and peeled off by means of an elevator The tumour, which involved the whole of the right lobe of the prostate, was thus removed piecemeal by cutting forceps and curved scissors, only a thin layer being left to support the mucous membrane of the bladder and prostatic urethra, which was left intact. There was very little bleeding, and this was controlled by forcipressure, no vessels being ligatured. On completion of the operation a soft rubber perineal tube was passed through the urethral opening into the bladder and retained there by a suture, and on this a rubber drainage tube was fixed to convey the urine to a vessel beneath the bed. The large gaping wound in the ischio-rectal fossa was packed with iodoform gauze and the usual dressings applied.

The perineal tube was kept in for six days, during which time the whole of the urine passed through it, not a drop escaping beside the tube, thus obviating any chance of infection of the deep ischio-rectal wound till it had partly filled up and was covered by healthy granulations. The temperature rose to 100° F. on one night, after which it did not go above 99° F.

After a fortnight the urine began to pass partly through the urethra, and after a few days later no urine passed by the perineal wound in the urethra. The patient was sitting up on December 1st. On the 18th he left the surgical home, untroubled by any urinary symptom, and he is now quite well.

The tumour turned out to be of the fibro-adenomatous type, frequently met with in the prostates of the aged, the fibrous elements predominating. It was as hard as scirrhus. About its centre a few drops of pus were found in the middle of a small coagulum, due to a thrombus or irritation of a small prostatic concretion found in this vicinity.

The advantages claimed for this method of operating are :—

1. The preliminary external urethrotomy permits of the introduction of the finger into the bladder, which, with a finger in

the rectum, enables the surgeon to define accurately the shape, density, size, and extent of the prostatic growth, in a manner that cannot otherwise be accomplished, except, perhaps, by a supra-pubic opening—a much more serious proceeding.

2. The finger can be hooked over the enlarged lateral lobe and the latter pushed well into the ischio-rectal wound, thus to a large extent obviating a deep and dangerous dissection, as in Dittel's operation.

3. With the tumour pushed well into the wound the capsule is easily incised and erased, and the cutting forceps and scissors easily and freely applied, to eradicate the whole mass.

4. The finger in the bladder enables the surgeon to feel when the cutting instruments are approaching that viscus, so that he can remove the whole growth, except a thin layer for the support of the mucous membrane of the bladder and prostatic urethra, without opening the latter, thus avoiding infection of the wound and the existence of a permanent fistula.

5. The stout perineal drainage tube inserted into the urethral wound carries off all the urine and prevents the perineal wound from becoming septic.

The drawback to the operation is that, like all other operations proposed for the removal of prostatic growths, it is applicable only to certain forms of the disease. It is not, of course, applicable to cases of enlarged middle lobe projecting into the cavity of the bladder. For such cases McGill's operation is undoubtedly the best. In very fat patients, or when the prostatic tumour is extremely large, it may not be feasible, owing to the fact that the finger may not in such cases be sufficiently long to hook it round the growth.

Mr. Mansell Moullin observed that, for some time past, prostatectomy had been somewhat in the background, in spite of all that McGill did and the success that followed his operation. This might be due, to some extent, to the fact that other measures for causing atrophy of the enlarged prostate came in about the same time and had been practised very largely. He was sure that there were certain cases in which prostatectomy could be done with much benefit. Perineal prostatectomy, however, was only admissible where there was no intra-vesical growth, and where the perineal distance was not too great. In the author's case, the perineal distance must have been well within the reach of his finger. It could not be done if the perineum were very fat or rigid. When, however, the enlargement was confined to the lateral lobes or the median portion—not the part that grew up into the bladder, but the true median

portion—he thought perineal prostatectomy was practicable. He admitted that the mortality was still somewhat high, as high indeed as 14 to 15 per cent., but that was due largely to the fact that operation had been postponed until the bladder was in an altogether hopeless state. With regard to the method described by the author, he remarked that he himself had performed an almost identical operation three months ago in almost the same way, but with this difference—that the author had planned his operation, while he himself had not. It was the result of one of those accidents which sometimes happened during operations. He had a patient with an enlargement of one lobe of the prostate, the other being almost normal. He had opened the urethra for the purpose of examining it, and had then enucleated the prostatic growth through the external opening, not from the urethral surface. He had not, like the author, introduced a tube, but packed the external opening with gauze. The patient passed his urine through the urethra almost from the first. Nicoll's operation was doubtless much more thorough, but it involved a suprapubic incision. He pointed out, however, that in patients not too fat, if the abdominal wall were fairly thin and the patient in the lithotomy position, it was possible to press through the abdominal wall down into the perineum, so that, without any incision, one could bring the prostate and bladder well into the perineal wound. By this means a very fair idea of the condition was obtained, though, of course, not such an accurate sensation as by putting the finger into the bladder. He had only one objection to the paper, viz., that as the author's patient was 59 years of age and his symptoms had developed some three years before—the enlargement presumably having begun before that—he did not think that changes taking place at that age deserved to be called "senile."

Mr. Swinford Edwards objected to the title of the paper, which, he thought, ought to have been "Removal of tumour of prostate by para-rectal incision combined with external urethrotomy." In his list of operations the author did not mention one method with which he himself was most familiar, viz., urethral prostatectomy by means of Gouley's prostatome. This was shaped like a lithotrite, and by its aid a low-level gutter was made in the prostatic urethra. This instrument was a later invention than Mercier's, which merely made a clean incision, but did not remove sufficient tissue to give a good permanent result. Another method not mentioned by the author was that introduced by Casper, of Berlin, who performed electrolysis of the tumour through rectal punctures. Still another method was electrolysis of the prostate by means of a bulbous electrode lying in the prostatic urethra. There were so many methods that it was evident that no one was applicable in all cases; in fact, most of them had been put aside as ineffectual. With regard to the author's case, no doubt this method answered very well, as it did in all cases where a tumour of the prostate had to be dealt with, and where removal was sought without impinging upon the mucous membrane covering it towards the bladder. For most cases of enlarged prostate, where intravesical overgrowth formed an obstruction to micturition, McGill's was the best operation. It was attended with a certain amount of risk, but it was the most radical.

Mr. Stanley Boyd agreed that the title of the paper ought to have been "Removal of a tumour from the prostate," but the incision should have been described as circum-anal. He pointed out that such an incision, carried into the ischio-rectal fossæ, must divide the inferior

hæmorrhoidal nerves, and destroy the sensitiveness of the anus; and he asked whether any undesirable results followed from this in the author's patient. On theoretical grounds, he was tempted to ask whether a much better access would not have been given by turning back a large semi-lunar flap of skin and fat, the base to extend from one tuberischii to the other, and the free margin to pass in front of the central point of the perineum. The posterior surface of the prostate could thus be easily exposed, and a median section of it could readily be done. As all fat would be turned down, the depth of the perineum would be greatly reduced. Mr. Boyd did not see why an enlarged median lobe could not be dealt with in this way, as the inferior hæmorrhoidal and long perineal nerves would be spared.

Mr. Bruce Clarke observed that the number of operations that had been suggested for dealing with cases of enlarged prostate showed that none of them could be regarded as ideal, nor was it difficult to see why this was the case. Enlargement of the prostate took place in three different directions: either through the bladder, in which case it could not be dealt with through the perineum; or into the rectum, in which that course was possible; or else in the median direction, when, to pressure upon the prostatic urethra, difficulty of micturition supervened. It was also a matter of considerable difficulty to know how much one ought to remove. He had seen some of the operations referred to by Mr. Edwards. They effected their object for a time, but by such means enough could not be punched out. If this method was to do any good, a piece ought to be removed every six months—a prospect not likely to commend itself to the patient. It was useless to suppose that they could, by any one method of operation, get at all these various enlargements of the prostate. He suggested that prostatectomy was not more popular simply because they had not hit off a plan that would insure success in a reasonable proportion of cases. This lack of success was due partly to sepsis and partly because it was difficult to determine beforehand what had to be removed. Until they could eliminate sepsis, and make out exactly what had to be done, he did not anticipate a much greater measure of success.

The President observed that cases of prostatic trouble did not always go to the surgeon at first. They sometimes went to a physician, and he had been consulted about such cases. With regard to the remark as to the operation being unsatisfactory on account of the septic condition of the urine, he admitted that possibly both consultants and general practitioners kept these cases in hand too long before sending them to the surgeon. That, he observed, was a very important aspect of the subject to be borne in mind in dealing with them. He objected to changes at or about 60 being described as "senile," for many persons presented no signs of senility at that age.

Mr. Freyer, in reply, agreed that his operation was only applicable to growths of the lateral lobes of the prostate. He pointed out that, in his case, there was a growth the size of a walnut projecting into the bladder. When the growth projected wholly into the bladder, then he preferred McGill's operation; but if the tumour projected, as in his own case, not only into the bladder but also into the deep perineum, he thought it could certainly be removed by the method he had indicated, and within the limits he had laid down. In designating the disease "senile enlargement of the prostate," he had followed Sir Henry Thompson's nomenclature. He pointed out that these growths were not always

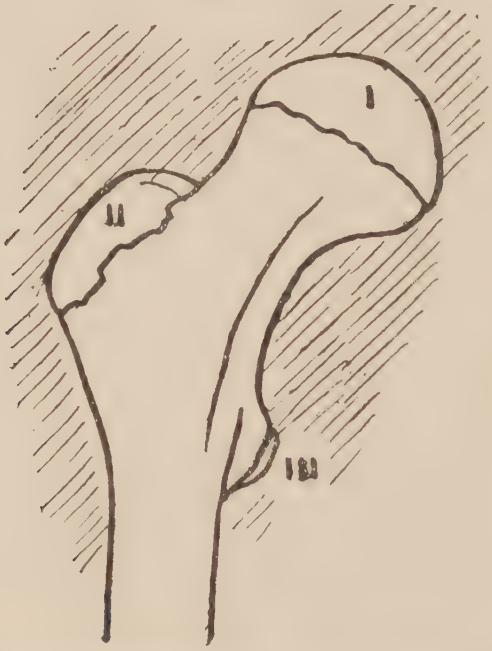
hypertrophic; there might be no hypertrophy of the glandular tissue at all, but merely of the nature of pure fibroma. In answer to Mr. Edwards, he said that he had only referred to the more important operations, and had, therefore, omitted some. It had been remarked that the operations referred to by Mr. Edwards did not take away enough, but his own objection to them was that they were likely to take away too much, for in these operations one was working in the dark, and dealing with a disease admittedly protean in its forms. The same objection applied to Bottini's operation. With reference to Mr. Boyd's suggestion, he had no doubt the prostate could be removed in the manner described, but they were now dealing with practical operations that had actually been done, and the result of which they were in a position to gauge.

COXA VARA.

By C. B. Keetley, F.R.C.S. Eng.

Coxa vara may be defined as a deformity of the upper epiphyseal region of the femur, in which the head of the bone sinks to a lower level than normal, in extreme cases almost touching the

Fig. 1.



The upper epiphyseal region of the femur; I, II, III, epiphyses. (From a specimen in the Museum of the Royal College of Surgeons of England.)

lesser trochanter. Coxa vara has also been named "curvature of the neck of the femur," but it is not likely that so short and handy a name will be displaced by a phrase including seven words

and not less open to criticism. In coxa vara the upper border of the neck of the femur is longer than normal, and the lower border shorter than normal (*see* Figs. 2 and 3). The upper border may be as much as three times the normal length. A

Fig. 2.

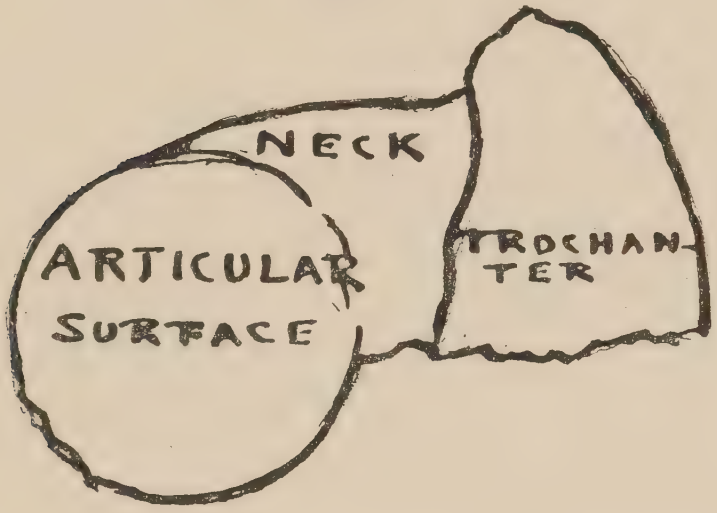


Fig. 3.

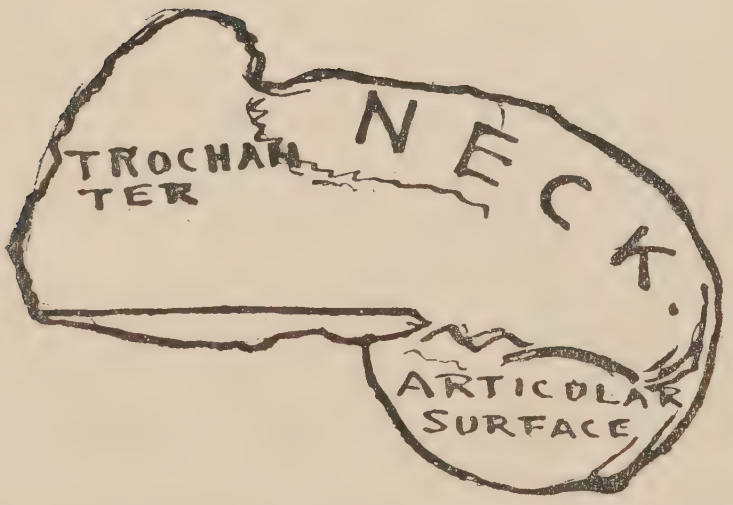


Fig. 2 and Fig. 3 respectively posterior and anterior diagrammatic views of the neck, &c., of a femur affected with coxa vara. (After Kocher, 'Deutsche Zeitschrift für Chirurgie,' 1894, Band XXXVIII, p. 536.) Note the articular surface looking backwards and downwards instead of inwards and upwards.

necessary result is that the neck is more horizontal than normal, or it may descend instead of ascending in the usual way. The articular surface of the head may thus come to look downwards. There is another remarkable and practically important change.

The neck is bent in a horizontal as well as in a perpendicular plane, with the convexity forwards (Figs. 4 and 5).

Coxa vara does not consist of a mere diminution of the angle formed by the long axis of the neck with the long axis of the shaft of the femur. Indeed, the changes are not confined to the neck. They always involve the head, and sometimes extend some

Fig. 4.

Fig. 5.

Similar specimen to that sketched in Fig. 2 and Fig. 3, but viewed respectively from above and from below. (Pen-and-ink sketch after Kocher's engravings, showing the horizonta curvature of the femoral neck, which limits the range of inversion.

distance down the shaft, just as in genu valgum the deforming influence often affects and curves the lower third or half of the shaft. The horizontal change is perhaps not constant. It manifests itself clinically by producing eversion and diminished range of inversion of the limb. Hofmeister arranged a collection of 53 cases of coxa vara into three groups :—(1) With no

outward rotation; (2) with outward rotation; and (3) with inward rotation (rare, only three cases). No one but Hofmeister himself had met with examples of this third group. In one of his cases one limb belonged to group 1 and the other to group 3. When a perpendicular section is made, the strongest and most compact part of the bone is seen to be not the lowest part of the neck but the upper part, near the epiphyseal cartilage, on both the distal and proximal side of that cartilage.

The articular cartilage is sometimes thinned towards the centre of the head. Usually there are no osteophytic growths, and, with the exception of my own first case, no recent signs of rickets have been seen on microscopic examination, perhaps because the rachitic process was over, though leaving the deformity behind. A microscopic examination of the wedge of bone removed from my case was made by Dr. W. S. Colman, then house physician at the West London Hospital. It showed "exactly such changes as are seen in bones known to be affected with rickets." Very frequently other, probably rachitic, deformities co-exist. The co-existence of scoliosis and genu valgum suggested to me the correct diagnosis in my first case.

Writers who do not believe in the relation of rachitis adolescentium to coxa vara seem to have curious ideas of what are the usual symptoms of the former affection. For instance, one author says of a case:—"No evidence of rickets; genu valgum present on both sides"! Surely the conjunction of coxa vara and genu valgum is itself evidence of rickets, though, of course, not conclusive.

Adolescents attacked with rickets do not present the same clinical picture as infants. They are not visibly rachitic in every limb. They are not pot-bellied, bow-legged, beaded-ribbed, &c., any more than they are unable to walk or collapse in the spine when they sit down. The older a person is when attacked with rickets the more limited and localised are his resultant deformities likely to be.

Deformity of the upper end of the femur, including increased angularity of the neck, has been recognised for many years as a common symptom of general rickets in young children. Jenner referred to it in his lectures. What has attracted so much attention in the last 11 years is the deformity when observed in adolescents.

I believe that in many, if not in most, cases of genu valgum the knee deformity is a compensatory curve to a certain degree of coxa vara. It would take too long a digression to give reasons, but I would invite anyone interested in the question to examine the rachitic skeletons preserved in our museums.

Another thing which I have twice distinctly noticed in cases of my own, and which I believe I can trace in the portrait given by Kocher of one of his cases, is an alteration in shape of the bones of the face and forehead. I do not like to call it a deformity. It is not necessarily unpleasing. It is most marked in one case (Fig. 8). The facial bones seem thin and expanded, especially in the orbital and nasal regions. I do not say they *are* thin, only their appearance of expansion suggests it. The orbits are shallow and the eyeballs are inclined to be prominent. The ridge of the nose does not rise sharply. In fact, there is a lowness of relief about some of the features, but something essentially different from the ordinary "flat face."

Symptoms.—Pain in the hip, brought on or increased by walking, is almost always the first symptom noticed. This pain may extend down to the knee, which is perhaps one reason why these cases have been so often confounded with hip disease. Soon afterwards the patient begins to limp, and soon tires on walking. In bilateral cases the second hip is generally attacked soon after the first, *e.g.*, at an interval of three or four months. The usual age at commencement is from 12 to 18 years, especially about 15 years of age. Eventually the objective signs appear. The trochanter rises above its proper level, usually with shortening of the limb as a result. Abduction becomes limited or abolished at the hip, flexion also, as well as internal rotation. The foot is generally more or less everted. Genu valgum appears. It is probably a compensatory curvature. Flat-foot and lateral curvature of the spine are occasionally seen. In severe cases the movements of the hip become extremely limited, so that it may easily be supposed by mistake to be the seat of fibrous ankylosis. In a small proportion of cases external rather than internal rotation is reported to have been limited. Sometimes flexion is comparatively free. The range of flexion can sometimes be increased by everting the limb. Passive mobility generally seems to be greater than active mobility. As is apt to be the case with ordinary flat-foot, the venous circulation is often not good, and

the extremities tend to be rather cold and blue when allowed to hang down. When the thigh is much adducted, the knees are thereby brought one in front of the other. It is then especially that genu valgum is likely to co-exist. A single-hip case limps, but when both hips are affected the sufferer waddles, shuffling one knee round the other as he walks. When the hips and knees are flexed in such a case the thighs cross.

Diagnosis.—My first case, the subject of which had visited various surgeons and hospitals, had been diagnosed as respectively periostitis of the femur (the trochanter was broader as well as higher than normal), sarcoma of the femur, and dislocation of the femur on the dorsum ilii. Other cases have been diagnosed as "arthritis adhesiva." I am afraid I do not know the meaning of this term, unless it means ankylosis secondary to inflammation of the joint. According to Frazer, it was the diagnosis made by Kocher of his first case before he had excised the joint and had seen the nature of the case explained by the specimen. I have seen a case exhibited at a society as one of separation of epiphysis, and when I asked for the points which distinguished it from coxa vara the distinguishing symptoms pointed out to me were exactly those of coxa vara. This case, or rather the skiagrams of it, are published as a case of separation of epiphysis. I have tried, but unsuccessfully, to see the patient. The waddling gait of congenital dislocation may suggest coxa vara. But the looseness of the joint in most cases of congenital dislocation contrasts with the stiffness of coxa vara. Coxa vara contrasts with hip disease in the following points. Gentle passive movements and pressing the head into the acetabulum are not painful. There are no startings at night. As a rule, wasting of the thigh is much less marked, though not absent. The head can be felt in the acetabulum, however high the trochanter may be, and it is not tender to direct pressure. Pain is easily relieved by rest without fixation. Roentgen rays are of the greatest value in finally settling the diagnosis. The skiagram should be studied in connection with the general clinical picture presented by the case. Nothing is more misleading than a skiagram studied by itself.

Prognosis.—The disease is progressive at first, but it tends ultimately to come to a stop. The deformity remains. In the worst cases not only is there great shortening, but the joint is so stiff that the patient has difficulty in lacing his boots and

in sitting down comfortably, especially on a low chair, and is easily fatigued by walking. The general health does not suffer, except indirectly through pain and want of exercise.

Treatment.—An obvious indication is to restore, as far as possible, the natural shape of the bone by osteotomy. Another is to relieve the pain by rest. A third is to increase the range of movements by passive exercises and even by forcible movements under anæsthesia. One of my patients said that he was thus benefited by a bone-setter, who, I need not add, was quite mistaken as to the nature of the case he had to deal with. Palliative measures are rest in bed, with or without extension. Two or three weeks generally suffice to remove the pain and some of the stiffness. In old-standing cases iodide of potassium and salicylate of soda, as well as the systematic use of purgatives and massage, might be also tried. I am referring to middle-aged patients, or at least to cases of, say, 10 years' standing in which there is sometimes a suspicion of complication by gout or rheumatism. Bicycling was of great benefit to one of these cases (a unilateral coxa vara of 25 years' duration). In the early years of the case sea-air, Parrish's food, and other anti-rachitic remedies should be tried. It is impossible to say when such treatment ceases to be useful. Although I think that the influence of weight pressure is a factor that has been greatly exaggerated in explaining this deformity, carrying weights to the extent of causing fatigue should be avoided, and if the patient's occupation necessitates this he had better change it. None of my own patients followed occupations requiring them to carry weights. The relief of pain and stiffness by rest is apt to prove of only temporary value. Both are liable to recur after exposure to fatigue or on the occurrence of damp weather. Massage is frequently recommended. It can at best only relieve aching and lessen stiffness. It cannot affect the deformity.

Operative Treatment.—In some of the earlier cases a mistaken diagnosis led to the excision of the head and neck of the femur and part of the trochanters. As the joint is practically healthy excision is now pretty generally condemned. The original excisions served a useful purpose, however, because they furnished the specimens from which first Müller, and afterwards Kocher, gave accurate accounts of the anatomy of the disease. What is to be thought of reports like the following (a reference to one

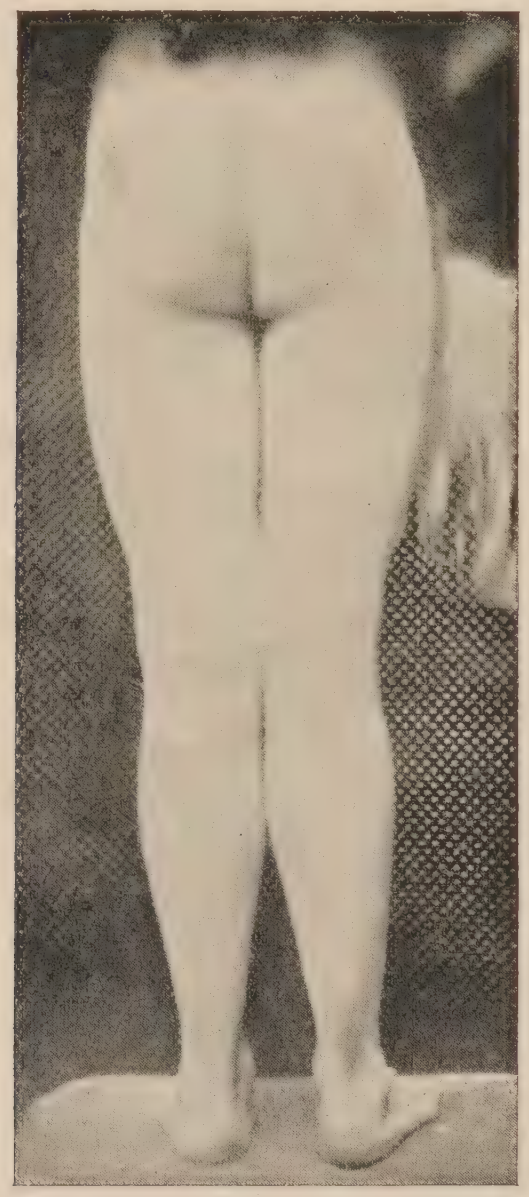
of Schneider's cases) ? " Patient limps, but has no pain. Treatment: resection of the hip joint. The patient was discharged

FIG. 6.

From the case described by me in the 'Illustrated Medical News' for September, 1888. Note the adduction at the left hip, the genu valgum, the eversion and the shortening. The prominence at the hip is not the great trochanter, but is due to a curve in the shaft below. The great trochanter could be felt much higher and considerably above the level of the head of the femur, which could be felt in the normal situation. I performed a subtrochanteric wedge-osteotomy on this case in 1888. A skiagram of this case demonstrates in the hip the characteristic changes of coxa vara.

cured." Cured! What of? Not of the limp, we may be sure; not of the adduction either, unless bony ankylosis ensued, nor of the shortening. Increased mobility may have been obtained, but at the expense of increased weakness and diminished length.

FIG. 7.



From a youth, aged 18 years, the head of whose right femur was about on a level with his great trochanter. The shortening of the limb was almost completely compensated for by increased length of the leg below the knee. There were well-marked symptoms of coxa vara from the age of 14 years.

The practical question at present is how and where to perform osteotomy. "Kocher, Hofmeister, and others have advised a subtrochanteric osteotomy," writes Frazer. He might have

added that subtrochanteric osteotomy had been performed for coxa vara some years before by myself. It has recently been done and strongly recommended by Watson Cheyne. I removed a wedge. The patient herself was much pleased with the result, mainly because it greatly diminished the shortening. Watson Cheyne directed his attention chiefly to remedying the eversion and effected great improvement. He used a silver plate and steel pins to fix the fragments in the corrected position. The seat of the deformity being mainly above the trochanter the obvious indication would be for supra-trochanteric osteotomy, but for the comparative depth and difficulty of controlling the upper fragment, consisting only of the head and part of the neck, after that operation. It has been recommended by Kraske, whose

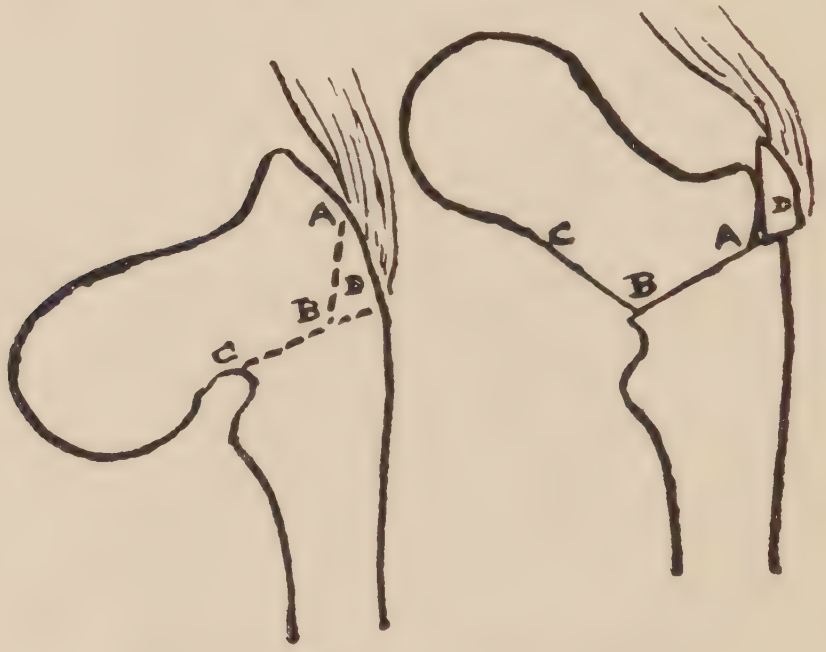
FIG. 8.

Shows the face of the patient in one of my cases of coxa vara. The changes are exceptional, but I have seen similar, though less marked, features in another case. They probably only occur when the rachitis has begun at an earlier age than adolescence.

operation consists of the removal of a wedge from the neck, with the base upwards, through an anterior longitudinal incision. Budinger says that linear supra-trochanteric osteotomy does just as well if the limb be kept well abducted and everted during the after treatment. My experience of osteotomy in general is that if the operator be a practised osteotomist a better result can generally be obtained from a wedge than from a linear osteotomy, except where, as in Macewen's operation for genu valgum, the bone is comparatively deep and slender, and the correction desired very simple. The neck of the femur is deep and slender, but the correction desired is not simple. The abnormally great length of the upper border of the neck of the femur invites

strongly to a wedge osteotomy. But it must be remembered that the lower border of the femoral neck is also abnormally short. No operation hitherto performed would cure the shortness, which constitutes a part of the deformity which is of great practical importance. If the problem were merely one of joinery such a procedure as the following might be adopted. The femur would be divided obliquely, from without inwards and downwards, just above the lesser trochanter. The outer surface of the upper fragment (D, Fig. 9) would be cut off and turned upwards with the muscles, which would be left attached to it. Then the

FIG. 9. FIG. 10.



Illustrations of a suggested operation. *Vide* text.

surface A B (Fig. 9) would be brought down into the original position of B C, and fixed there with pins. The result as shown in Fig. 10 would be to bring back the articular surface of the femoral head almost into its normal position. To make the articular surface look sufficiently forward—*i.e.*, to remove the evil effect of the horizontal curve in the femoral neck—a wedge with the convexity forward would also have to be removed from either the lower fragment at B C (Fig. 9) or the upper at A B (Fig. 9). Otherwise the external rotation of the limb would have to be corrected by a separate simple osteotomy, done after the first osteotomy was recovered from. Mere rotation inwards of

the limb would disturb the fit of the fragments if done as a part of the first osteotomy. But surgery is not mere joinery, and such an operation as I have sketched out would never be popular with practical surgeons. If carelessly or awkwardly done there would be risk of imperfect fixation and even of splitting the bone in fixing the pins. Nevertheless, the operation is a practicable one, and, properly executed, would almost certainly give a first-class result, better than any got from simple osteotomy. Bold division of soft parts, especially joint-capsule, would be required. Pins are extremely useful in fixing bony fragments after either fracture or osteotomy, but they should never be trusted to alone. They should be properly supported by splints, bandages, and extension apparatus, and the action of these should be carefully superintended. The pins should be of thickly silver-plated steel. The holes for them should be bored by sufficiently large gimlets or American bits, as the insertion by force of pins into small holes will result in splitting the bone. Of course, when the bone is soft and tough and the pin small in diameter a bradawl suffices for boring.

Historical.—My chief object in adding a few words under this head is to make a small claim for priority. The position as regards this point was clearly and concisely put by Ernst Müller, of Stuttgart, in the 'Centralblatt für Chirurgie' of September 1st, 1894. His first work on the subject appeared in Bruns's 'Beiträge zur Klinischen Chirurgie,' published in November, 1888. He reported therein two cases in which the head, neck, and part of the trochanteric region of the femur had been excised. He gave a very good description of the clinical histories and of the specimens, and expressed the opinion that the affection was of rachitic origin. Afterwards von Lauenstein, Hoffa, Rotter, all in 1890, and later Hofmeister and others, published additional cases and described fresh specimens. Then Kocher in 1894 published a paper—"Ueber Coxa Vara eine Berufskrankheit der Wachstumperiode"—of the highest intrinsic value, but doing scant justice to Müller and others. Müller wrote in the 'Centralblatt für Chirurgie' of September 1st, 1894, p. 818: "Even if Kocher had not read my work in the original the others (viz., the papers of Hoffa, Lauenstein, &c.), which all referred to mine, could not have escaped his observation." And further: "Kocher had, therefore, no right to assert that he published his cases

without knowing anything about my work, since otherwise he lays himself open to the reproach of not inspecting (surgical) literature, which is the first thing which an author should do before he describes a new disease." Kocher defended himself with his usual ability, but not, I think, with complete success. He persuaded himself (I think erroneously) that his cases differed essentially from Müller's, and that because they presented at the hip certain analogies with the changes at, and near, the ankle in talipes varus only cases precisely like his ought to be called "coxa vara."

I must now point out that Müller himself could scarcely have exhaustively "inspected surgical literature" before publishing, because what, to him at least, should have been obviously a case of coxa vara was published by me in the first number of the 'Illustrated Medical News' (of date September 29th, 1888). Müller's first publication appeared in the following November. My paper was entitled: "A Case of Rachitis Adolescentium, in which the Disease was for Several Years Localised in the Trochanteric and Infra-trochanteric Region of the Right Femur, and afterwards attacked the Spine; Wedge Osteotomy for the Femoral Deformity and Plaster-of-Paris Jacket for the Spinal." In the text I wrote of the case that it was "one of *rachitis adolescentium, attacking first the upper epiphyseal region of the femur* (*see* Fig. 1), and secondly, after some years, the epiphyseal regions of the vertebral bodies." A photograph attached shows very plainly the adduction of the hip, the eversion of the foot, the shortening, and the compensatory knock-knee (Fig. 6). My case was the first, therefore, in which the nature and seat of the disease were diagnosed correctly during life and before operation.

There are several reasons why I have not hitherto received credit for this: (1) The next observations were made in Germany; indeed, for some years the literature of the subject was purely German. (2) I did not make the mistake of diagnosing the affection to be one of the hip-joint and then, by excising the upper end of the femur, put myself in a position to give an anatomical description of the specimen. (3) I omitted to give any clinical account of the objective symptoms and left the photographs to speak for themselves. (4) The art editor of the journal added a misleading diagram "to explain the nature of the *operation*." I pointed out its erroneous nature in the last

paragraph of my paper, but in terms rather too mild. Lastly, the journal in question only lived two years; its production was found to be too expensive. It was the largest and most copiously illustrated medical journal of its time. Various Continental and American surgeons have written to me asking for the loan of the journal, but the bound volume in my possession was too cumbrous to send abroad.

Previously to the date of my case Dr. Monks had given an excellent clinical description of a case which was most likely one of coxa vara, and he had, moreover, distinctly recognised that the trochanters had risen above their normal level with regard to the heads of the femora. He had, however, believed that the femoral heads were also dislocated, and that the cause of the changes was rheumatoid arthritis. Assuming the case to be one of true coxa vara I think Dr. Monks must have been mistaken on these points. Various pathological specimens had also from time to time been preserved or described, but had been supposed to be due to rheumatoid, or to inflammatory changes, or to traumatism.

Lastly, the existence of coxa vara in rachitic children had been long known—indeed, it could be seen in almost every pathological museum of importance. Jenner referred in his lectures to the great frequency with which the upper extremity of the femur is deformed in the rickets of childhood. It was, however, reserved for Ernst Müller to give the first complete description of coxa vara, and Kocher's paper is so valuable from the exactness and fulness of its anatomical descriptions that, with or without priority, it must always remain a classic. The fullest account in the English language is probably Frazer's in the 'Annals of Surgery' for June, 1899, and the most elaborate in any language is Hofmeister's (*op. cit.*). Whitman, Bayer, Kirmisson, and others have made valuable contributions, and, as well as Frazer, they give references to the literature of the subject.

March 26th, 1900.

THE GELATINOUS FORM OF SODIUM BIURATE AND ITS BEARING ON THE TREATMENT OF GOUT.

By Arthur P. Luff, M.D., B.Sc., F.R.C.P. Lond.

I do not propose in this paper to touch upon the debatable questions of the seat or seats of formation of uric acid, or of the compounds from which that body may be derived. I believe, however, that it is a practically indisputable fact that the deposition of a salt of uric acid—namely, sodium biurate—is intimately related to the gouty attack, and I propose to briefly bring forward to-night the results of some experiments which I think throw some light on the therapeutics of gout.

I strongly share the view of the late Sir William Roberts that uric acid, when first introduced into the blood, exists solely therein as the sodium quadriurate. This substance is, in the dissolved state, a very unstable body, and soon changes into the sodium biurate, which first assumes the gelatinous and soluble form. This gelatinous modification, if not eliminated in the urine, is converted either slowly or rapidly, according to various conditions, into the crystalline and almost insoluble variety, which is then precipitated, and, by its deposition in various structures, causes the gouty paroxysm. It is obvious that if by any means the conversion of the soluble gelatinous biurate into the insoluble crystalline form can be delayed, then the advent of the gouty paroxysm is also delayed, and if, during this period of delay, the elimination of the soluble gelatinous biurate is promoted, then the gouty paroxysm may either be considerably diminished in severity or may even be averted altogether.

I have for some time been studying the physical and chemical properties of this gelatinous form of the biurate, and especially the effects exerted by certain drugs on its conversion into the crystalline form. The drugs, the influence of which on the gelatinous biurate I have experimentally investigated, are sodium bicarbonate, sodium phosphate, potassium bicarbonate, potassium citrate, lithium carbonate, lithium citrate, piperazine, and lysidine. In a paper which I communicated to the last meeting of the

British Medical Association,* I showed that by increasing the alkalinity of blood serum with sodium bicarbonate the conversion of the gelatinous biurate into the crystalline form is accelerated in time and increased in quantity, whereas when the alkalinity of the serum is increased by the addition of potassium bicarbonate, the conversion of the gelatinous biurate into the crystalline form is delayed as regards time, and is considerably diminished as regards quantity. I have now carried out a similar series of experiments with the other drugs previously mentioned.

Briefly stated, the results show:—(1) That sodium salts considerably accelerate the conversion of the gelatinous biurate into the crystalline variety, and that their employment in the treatment of gout is apparently not desirable; (2) that potassium salts delay the conversion of the gelatinous biurate into the crystalline form, and also that when the conversion is once started it is slowed by the presence of these salts; (3) that lithium salts, although they do not delay the initial conversion of the gelatinous biurate, yet when the conversion is once started it is slowed by the presence of these salts, and especially by the lithium carbonate; (4) that piperazine does not delay the initial conversion of the gelatinous biurate, and but slightly slows the conversion when once started; and (5) that lysidine, although it delays the conversion of the gelatinous biurate into the crystalline form, yet when the conversion is once started it has practically no effect in slowing it. From the results of these experiments it appears that for the special purpose referred to in the treatment of gout the potassium salts are the most useful, that the lithium salts rank next, and that piperazine and lysidine are not nearly so useful. I may mention that these results are entirely in accord with my clinical experience.

I do not for one moment contend that the beneficial action of the salts of the alkali metals in the treatment of gout is to be solely gauged either by their solvent action on deposits of sodium biurate, or by their inhibitory action on the conversion of the gelatinous biurate into the crystalline form. Undoubtedly many of them are extremely useful in the gouty state by their stimulating effect upon metabolism, by their remedial action upon the gastric and hepatic functions, by their diuretic action, and by their

* 'British Medical Journal,' vol. ii, 1899, p. 1163.

diminishing the acidity of the urine. I should like to remark here that the purport of a paper published by me in the 'Lancet' of June 11th, 1898, has been somewhat misunderstood. In that paper I showed by experimental evidence that potassium, sodium, and lithium salts, and piperazine and lysidine exercised no appreciable solvent action on gouty deposits. I did not, however, question—still less deny—the utility of many of these drugs in the therapeutics of gout, for I am constantly employing many of them in the treatment of various phases of that disease; but I wished to point out that the oft-repeated statement that most of these drugs are useful for their great solvent action on gouty deposits is a loose and erroneous statement which it would be wiser in future to avoid.

Another important point demonstrated by these experiments is that the higher the alkalinity of the blood from the presence of sodium bicarbonate, the more rapid and the more complete is the conversion of the soluble gelatinous biurate into the comparatively insoluble and crystalline form. This point is of great interest, since I believe that the alkalinity of the blood of gouty patients is always higher than the normal alkalinity of blood. I know that this view is opposed to the prevailing view that gout is associated with what is called an "acid dyscrasia"—whatever that may mean—and that consequently the alkalinity of the blood of gouty subjects is below the normal. This view, however, so far as I can ascertain, is a pure assumption, and has apparently never been based upon any actual determinations of the alkalinity of the blood of gouty individuals. It received its first shock five years ago, when Klemperer showed by actual determinations of the blood of gouty subjects that the alkalinity was not below that of healthy blood. I have since made a number of determinations of the alkalinity of the blood of healthy and gouty individuals, with the result that I have so far found in every gouty patient whose blood I have examined that the alkalinity is higher than the average alkalinity of the blood of healthy persons. The results that I have up to the present obtained show that, whereas the alkalinity of the blood of healthy adults varies from 0·161 to 0·185 per cent. of sodium carbonate, with an average of 0·167 per cent., the alkalinity of the blood of gouty patients varies from 0·193 to 0·251 per cent., with an average of 0·217 per cent., which is nearly one-third higher than that of normal blood. Now since

this increased alkalinity of the blood is due to a higher proportion of sodium carbonate or bicarbonate, and since these substances both accelerate the conversion of the gelatinous biurate into the crystalline variety, and hasten the precipitation of the latter, it is intelligible why such blood is prone to hasten and to augment the formation of gouty deposits.

THE ACTION OF VARIOUS DRUGS AND DIETS ON THE EXCRETION OF NITROGEN IN GOUT.

By William Bain, M.D., M.R.C.P. (Harrogate).

The study of the elimination of nitrogen in gouty cases is of special importance on account of the relationship which uric acid holds to gout, and the consequent view that gout is essentially a disease of disordered metabolism. The object of this series of observations was to determine, if possible, the variations in nitrogenous elimination occurring in a case of gout under ordinary conditions, and to note the effect of different diets and certain drugs on such variations. For this purpose urea, uric acid, alloxur base, and ammonia were all separately determined—the urea to serve as a measure of the total nitrogenous changes—the uric acid and alloxur base as a possible sign of modified production of these bodies, and the ammonia as an indication of the general amount of inorganic acid dealt with during the day's metabolism. With regard to the genesis of uric acid, there are two alleged sources :—

1. It may be a derivative of the alloxur base combined within the molecule of the nucleo-proteid of cells, or similar substances in the food.
2. It may be produced by synthetic processes in the liver or elsewhere from simpler nitrogenous compounds such as ammonia.

If the former view be the correct one, variations in the amount of uric acid excreted must simultaneously produce variations in the final waste products resulting from the disintegration of nucleo-proteid, one of which—phosphorus—we can follow. For this reason the phosphorus pentoxide excretion was also determined, but it must be remembered that the main amount of

phosphates excreted in the urine has been derived from the phosphates taken as such in the food. The amount of ammonia excreted has been shown to depend largely, if not entirely, on the excess of mineral acid over fixed bases available, and consequently fluctuations in its amount follow variations in the diet.

I am indebted to Dr. Barrs for the opportunity of investigating the following case of chronic gout :—

G. D., aged 65, a well-built man, 6 feet in height, and weighing 12 st. 4 lbs., who followed the curious vocation of tailor and dancing master, was readmitted into the Leeds General Infirmary on November 2nd, 1899. His family history revealed little of positive importance. An uncle had suffered from gout ; but while his father, dying at the age of 67, was believed to have shortened his life by over-indulgence in alcohol, he had never been affected by gout. There was, however, a history of rheumatism in his case. The patient's mother, aged 77 at the time of her death, was always free both from gout and rheumatism. The patient's past record was excellent. Apart from the affection for which he was admitted, and an attack of erysipelas 23 years previously, his health had been very good. The history of gout covered five years. At the beginning the patient attributed the swelling of the joints to the fact that his feet ceased to perspire profusely. The recurring attacks were ushered in by shooting pains, the joints becoming red, swollen, and very hot. These symptoms gradually subsided after two or three days, with exception of the swelling, which persisted. At the time of readmission his general health was good, and no disease of internal organs could be discovered. Gouty changes were marked both in the hands and feet, the enlargement due to deposits being indeed extreme. In the hands the metacarpo-phalangeal and first interphalangeal joints were mainly affected. The dorsum of the left wrist was occupied by a considerable swelling. The elbow of the same arm was affected at two points, there being a very hard swelling in connection with the ulna, and a softer one forming a cushion-like expansion of the apex of the joint. At the back of the internal condyle of the other elbow-joint was a nodulated enlargement equalling a filbert in size. The fifth metatarsal joint of the left foot showed a large hard swelling, and a similar enlargement existed on the outer side of the fifth toe, with a smaller one on the dorsum of the fourth. The lower part of the tendo Achillis was irregularly swollen. The small toe of the right foot was enlarged dorsally, a smaller swelling affecting same surface of the first and fifth tarso-metatarsal joints. The tendo Achillis was in a condition similar to that described in the case of the left foot. The patient stated that the joints gave rise to no pain, except when at long and irregular intervals an inflammatory condition supervened.

The case was interesting from the fact that although the first manifestations of gout presented themselves only five years before, the deposits described were so large as to render it almost unique. A feature of interest, from an etiological point of view, was the abstemiousness of the patient combined with

his active habits. Except for a slight hereditary taint, mentioned in the report, no possible clue in relation to its causation was ascertainable. The urine, repeatedly examined, was free from albumen, albumose, and peptone; and throughout the investigation was acid in reaction.

Methods of Analysis.—The urea was estimated by the hypobromite method and uric acid both by Hopkins's and Salkowski's processes. Schlösing's method was adopted for ammonia and titration by uranium nitrate for the P_2O_5. The alloxur bases were estimated by Salkowski's recent method described in a former paper on gout.

While under observation the patient was placed on the subjoined diets :—

Fixed Diet.—Cocoa, 1 pint; tea, 2 pints; milk, 1 pint; teacake, 2 ozs.; bread, 11 ozs.; butter, 1 oz.; dinner, 1 lb. 6 ozs., including 4 ozs. of meat and one apple.

Vegetable Diet.—This consisted to a large extent of peas and beans. Milk, 1½ pints; oatmeal porridge, ½ pint; pea soup, ½ pint; peas porridge, ½ pint; bread, 11 ozs.; butter, 1 oz.; peas, beans, and other vegetables, 1 lb. 8 ozs.

Animal Diet.—Milk, 1½ pints; lemonade, 1 pint; meat, 10 ozs.; bread, 10 ozs.; butter, 1 oz.; potatoes, 4 ozs., and one apple.

In addition to being dieted, the patient took the same amount of exercise each day.

The results obtained are tabulated on next page.

Referring first to the estimations made before the administration of drugs and the adoption of experimental diets—that is to say, under ordinary conditions—it will be observed that the fluctuations were well within physiological limits with the exception of urea, which was somewhat low, a common feature in connection with hospital patients, resulting, of course, from the small quantity of nitrogenous food taken; but in this case the alternative possibility of a nitrogen retention must be borne in mind. Taking into consideration the patient's age, the uric acid excretion must be regarded as normal. The bases also bore the usual relationship to uric acid. The estimation of the bases gains much interest from the important researches of Minkowski, who administered adenin to four dogs in doses varying from 0·25 to 0·5 gram several times a day. Three of the animals were killed about the fourth day, and the other died on the second day. After 2 grams vomiting ensued, and the other symptoms noted were violent action of the heart, a heaving pulse, and an increase in blood pressure. The urine contained albumen, leucocytes, casts, and epithelial cells. *Post mortem*

TABLE I.—*Showing the Analytical Details of the Urine in the 24 Hours.*

Date.	Diet.	Drugs.	Total Quantity in c.cm.	Specific Gravity.	Urea in Grams.	Uric Acid in Grams.	P_2O_5 in Grams.	Alloxur Bases in Grams.	Ammonia in Grams.	Remarks.
Nov. 3	Fixed diet		1,720	1,011	17·200	0·401	1·21	0·055	—	Temp. normal.
,, 4	,,		1,420	1,013	18·460	0·367	1·27	0·051	—	
,, 5	,,		1,360	1,014	21·760	0·480	1·37	0·061	—	
,, 6	,,		1,430	1,012	19·305	0·509	1·51	0·067	0·486	
,, 7	,,		1,095	1,018	18·615	0·409	1·20	0·050	—	
,, 8	,,		1,535	1,013	20·327	0·457	1·41	0·057	0·582	
,, 9	,,		1,435	1,015	21·455	0·476	1·48	0·048	—	
,, 11	,,		1,380	1,017	20·735	0·545	1·57	0·053	0·455	
,, 12	,,	Sodium salicylate, 20 gr. ter die.	1,555	1,017	24·824	0·529	1·59	0·075	0·509	
,, 13	,,		1,600	1,015	21·612	0·491	1·56	0·075	0·489	
,, 14	,,		1,170	1,019	20·061	0·544	1·70	0·058	0·457	
,, 15	,,		1,330	1,015	19·951	0·419	1·31	0·060	—	
,, 16	,,		1,673	1,012	20·076	0·601	1·68	0·061	0·615	
,, 17	,,	Guaiac. resin, 18 gr. ter die.	1,900	1,010	19·164	0·600	1·44	0·054	1·113	
,, 18	,,		1,800	1,010	17·864	0·708	1·36	0·051	0·724	
,, 19	,,		1,320	1,012	17·820	0·842	1·18	0·053	0·516	
,, 20	,,		1,850	1,014	20·905	0·749	1·38	0·057	0·757	
,, 21	,,		1,495	1,013	20·115	0·688	1·61	0·059	0·685	
,, 22	,,	Potassium iodide, 5 gr. ter die.	1,655	1,013	23·170	0·606	1·45	0·058	0·787	
,, 23	,,		1,505	1,013	21·070	0·547	1·44	0·052	0·613	
,, 24	,,		1,733	1,012	23·048	0·571	1·59	—	0·561	
,, 25	,,		1,620	1,015	24·312	0·701	1·62	—	1·046	
,, 26	,,		1,250	1,015	24·986	0·618	1·75	—	0·701	
,, 27	,,		1,002	1,015	19·038	0·453	1·48	—	0·530	
,, 28	,,	Vin. colchici, 15 m. ter die.	1,210	1,014	22·990	0·584	1·63	0·045	0·636	
,, 29	,,		1,813	1,011	22·263	0·552	1·59	0·068	0·738	
,, 30	,,		1,814	1,012	19·954	0·501	1·46	0·055	0·678	
Dec. 1	,,		1,645	1,013	22·207	0·595	1·64	0·053	0·672	
,, 2	,,		1,870	1,012	20·944	0·492	1·51	0·042	0·572	
,, 3	,,		1,450	1,015	20·385	0·476	1·40	0·044	0·394	
,, 4	,,	Quinæ sulph., 3 gr. ter die.	1,900	1,012	24·264	0·560	1·70	0·051	0·613	
,, 6	,,		1,680	1,013	23·522	0·573	1·54	0·048	0·504	
,, 7	,,		1,245	1,016	22·416	0·512	1·46	0·069	0·463	
,, 8	,,		1,175	1,016	19·387	0·501	1·53	0·051	0·407	
,, 9	Vegetable diet		1,465	1,016	22·172	0·498	1·77	0·054	0·572	
,, 10	,,		1,530	1,020	19·925	0·618	1·99	0·069	0·520	
,, 11	,,		1,975	1,014	21·725	0·711	2·66	0·073	—	
,, 12	,,		1,480	1,016	19·240	0·674	2·47	0·063	—	
,, 13	Animal diet		1,225	1,016	23·275	0·666	2·12	0·061	0·699	
,, 14	,,		940	1,016	23·623	0·458	1·47	0·039	—	
,, 15	,,		930	1,018	24·180	0·557	2·35	0·054	—	
,, 16	,,		875	1,020	23·625	0·543	1·88	0·058	0·609	
,, 17	Animal diet + tea		1,134	1,017	25·484	0·688	2·25	0·055	0·941	
,, 18	,,		1,010	1,018	23·735	0·649	1·11	0·057	0·709	Temp. 103°.

crystals of biurate were found deposited in the convoluted and Henle's tubules, but not in the glomeruli. There was also cellular infiltration of the connective tissue.

It is certainly striking that this train of symptoms, with deposition of biurate, should be experimentally induced by one of the alloxur bases, and this suggests that one or more of them may be a factor in the evolution of gout. As negative evidence of such a causative relation there is the further fact that uric acid has not been shown by experiment to produce any such symptoms—a statement of special significance as regards the circulatory disturbance. The extreme insolubility of uric acid also points to the possibility that, with an advancing knowledge of the pathology of gout, it may be relegated to a secondary position in the causation of that disease. Apart from xanthin, the amount of alloxur bases appearing in the urine is so exceedingly small that their separate estimation is a matter of great difficulty. Supposing, therefore, the excretion of adenin were diminished or increased, such a variation would only in a slight degree alter the total quantity of the bases; hence the fact of the bases varying within normal limits in proportion to the acid does not exclude the possibility of adenin being retained in the system, and thereby determining the deposition of biurate in the tissues. It is to be regretted that this line of research has not been extended; but pure adenin is almost impossible to obtain at present.

Regarding the supposed nuclear origin of uric acid which links it with the output of phosphorus pentoxide, its proportionate relation to that substance was not so consistently maintained as was anticipated from previous observation. On several occasions during the first fortnight through a misunderstanding there were slight but, as regards phosphates, possibly effective variations in the fixed diet. It is of course necessary, in order to observe the relation in question, that the diet should remain absolutely unchanged both in quantity and quality. Dr. Chalmers Watson, in a paper of much interest, has thrown doubt on the parallel variations in the excretion of these two products as tentatively advanced by me in a former communication. Possibly he overlooked the statement that in gouty cases retention is a perturbing factor. Another disturbing factor to which, however, no reference was made in my paper is excessive excretion of

uric acid such as follows the exhibition of eliminative drugs. An example of this will be found in the tables. As regards his case it was not stated that the patient's food was weighed, and further, potassium iodide was being administered during the major part of the investigation. It has occurred to me that the discrepancy may be dependent upon either or both of these circumstances. For the estimation of uric acid Dr. Watson used a modification of Salkowski's silver method. Silver having a greater affinity for iodine than for uric acid, it follows that unless sufficient silver were added to precipitate both the iodine and the uric acid a part of the latter would remain in solution. In Table II an average has been taken of the estimated constituents of the urine, the last three days' average under ordinary conditions being placed first for comparison.

TABLE II.

	Averages, in Grams.				
	Urea.	Uric Acid.	P_2O_5.	Alloxur Bases.	Ammonia.
Fixed diet (3 observations)	20·839	0·492	1·48	0·052	0·507
Sodium salicylate (3 observations) ...	22·165	0·521	1·61	0·069	0·485
Guaiacum (3 observations)	18·282	0·716	1·32	0·052	0·784
Potassium iodide (3 observations) ...	22·429	0·574	1·48	0·056	0·653
Colchicum (4 observations)	21·853	0·558	1·56	0·050	0·681
Quinine (2 observations)	23·892	0·566	1·62	0·049	0·558
Vegetable diet (4 observations)	20·565	0·625	2·24	0·064	0·546
Animal diet (4 observations)	23·675	0·556	2·00	0·053	0·654
Animal diet + tea (1 observation) ...	25·484	0·688	2·25	0·055	0·941

It is unnecessary to trace in detail the effect of the drugs given, as this is apparent in the tables. Contrary to expectation, after sodium salicylate there was only a small increase in uric acid, with a slight augmentation of the alloxur bases relative to the acid. Under guaiacum, on the other hand, while the urea fell somewhat, the uric acid was markedly increased; but neither the phosphorus pentoxide nor the bases showed a corresponding increase, thereby denoting that this drug probably acts not by increasing the production of uric acid, but by eliminating a part of that stored in the blood. The bearing of this fact on the treatment of chronic gout will be appreciated. It is also noticeable that the increased excretion of uric acid was maintained

for some time after cessation of the drug, the patient's condition being at the same time ameliorated. It is satisfactory to note that the experimental evidence adduced confirms the opinion, based on empirical knowledge, expressed by Sir Alfred Garrod that guaiacum was a powerful prophylactic agent in gout, this opinion being subsequently corroborated by Dr. Luff.

The raised excretion of uric acid with potassium iodide is probably attributable to the maintained effect of guaiacum. The value of potassium iodide in chronic gout is founded rather on its apparent power of retarding cardio-vascular and renal changes, and reducing albumen in the urine. Under colchicum a slight advance in the uric acid occurred. That it has an invariable effect on the excretion of uric acid is extremely doubtful. The result with quinine on the excretion of uric acid is contrary to the diminution usually observed. This again may be accounted for by the continued improvement in the patient's condition.

The results obtained with the experimental diets are not in agreement with the generally accepted view upon which the dietetic treatment of gout is frequently based. There is matter for reflection in the fact that a diet consisting largely of leguminous principles should so markedly increase the uric acid, the phosphorus pentoxide, and alloxur bases.

On the second day after tea was added to the animal diet the patient developed a temperature due to a chill which terminated the observations, so that no reliable inferences can be drawn from the record with regard to the influence of tea upon the urinary constituents estimated.

Dr. A. E. Sansom observed that he was still in a maze of doubt as to the precise pathology of gout, and contrasted some of the difficulties met with by the clinical physician with the valuable discoveries referred to in the papers just read. Everybody now conceded the deposit of urates in some parts of the body in certain forms of gout; and he asked how it was that these deposits were frequently taken up and got rid of in some cases, while in others they persisted. Then again, in many of these cases, there was a curious absence of pain, even in presence of unquestionable deposits of sodium urate in the tissues. Clearly, therefore, he could not clinically associate the deposit of sodium biurate in the tissues with the symptom of severe pain, that is to say, not regularly and unmistakably. In an acute paroxysm of gout it was difficult to resist the conclusion that there must be something beside these deposits of sodium biurate. He referred to cases in which the exceptional opportunity had been afforded of examining a joint affected by acute gout, in which an intensely destructive process was demonstrated which could not be

associated with the presence of sodium biurate alone. It was, doubtless, an unknown product of perverted metabolism which, like certain ptomaines with which they were acquainted, acted in very small quantities and produced distressingly violent effects. Great difficulty also arose in connection with the treatment. What some of them would consider a rational treatment might be explained in a different way to what they expected. If the sodium salts promoted the deposition of the crystalline form from the soluble quadriurate, while the potassium salts retarded this, how was it that a mixture of the two salts gave remarkably good results? Must they hold that they were wrong in prescribing the mixture? He thought not. He thought they were still a long way off the truth, and he repeated that the chief factor was a minute proportion of some unknown substance, from the perverted metabolism of which the manifestations of uric acid and the urates in the blood and tissues were the chief objective signs.

The PRESIDENT said that he was aware of the views held about the quadriurates, and had referred to the matter in his article on "Gout" in 'Quain's Dictionary of Medicine.' He was not acquainted with the different forms of the gelatinous and crystalline biurates, and Dr. Luff's contribution on this point struck him as being a very interesting and practical addition to their knowledge of the conversion of the one into the other, and of the influence of drugs in bringing this change about. His experiments established the value of their old friends, the potassium salts. He agreed that the combination of the sodium and the potassium salts gave good results. He thought that Dr. Luff's observations proved that piperazine and lysidine were not of much value in gout. He had himself seen a very similar case to the one related by Dr. Bain. His patient had remarkably gouty developments, with no particular causes to account for them. He was quite a young man, and there was only a slight hereditary tendency to gout. He was a butcher by trade, but had eaten very little meat, and was an abstainer from alcohol. Dr. Bain's observations on the effects of diet in his case were of much interest, and were well worthy of the attention of those interested in the pathology of gout.

Dr. LUFF, in reply, suggested that the disappearance of the deposits in some cases and their persistence in others might be due to the condition of the blood of a gouty subject in respect to alkalinity, viz., that if the alkalinity remained high, these deposits could not be dissolved on account of the high proportion of sodium carbonate, whereas if the alkalinity were diminished, then the deposits might undergo solution. With regard to the absence of pain in connection with some gouty deposits in certain patients, which was a matter of common experience, he pointed out that if the deposits were formed slowly there was no reason why there should be pain. To lead to the production of pain there must be a sudden deposition of sodium biurate to act as a foreign body. He instanced the slow formation of a gall stone which was not necessarily associated with any pain, and which contrasted with the suffering produced by the irruption of a small stone into the gall bladder or ureter. Therefore, to produce pain there must be a sudden and somewhat copious gouty deposit in the tissues. With regard to the acute necrosis alluded to by Dr. Sansom as having been met with in certain tissues in acute cases, he said he was very doubtful whether these were true cases of gout. He had never seen such a case in which necrosis of cartilage was associated with true gout. He believed the case referred to was probably one of rheuma-

toid arthritis, in which gout had supervened. He had certainly seen many cases of rheumatoid arthritis in which there were gouty deposits within the joints. This necrosis had been described by Ebstein and by Berkart, but he thought the cases were probably originally ones of rheumatoid arthritis with marked destruction of cartilage, and in which gouty deposits had subsequently occurred. He commented on the very painstaking way in which Dr. Bain had investigated his case, observing, however, that Dr. Bain had not told them for what relative lengths of time the patient had been put on the different forms of treatment. The one patient seemed to have had a great variety of treatment and, necessarily, his gouty condition must have been very much influenced by the previous treatment. [Dr. Bain explained that the patient was put on each of the drugs for three days, except colchicum, which was given for four days, with three days' interval after each drug.] He (Dr. Luff) had great confidence in the efficacy of guaiacum in many chronic forms of gout, in irregular gout, and also as a prophylactic ; but there was only one form in which it could be conveniently given, and that was as the powdered resin, in tabloids or cachets. He was pleased to see that Sir Alfred Garrod's and his own results were borne out by Dr. Bain's investigation on that point.

Dr. Bain, in reply, said, with reference to the absence of pain in association with the deposition of sodium biurate, he thought Sir William Roberts's explanation a satisfactory one, viz., that it depended, first, on the rapidity of deposition, and, secondly, upon the structures which were invaded by the crystalline deposit. If sensory nerves were involved, pain would be a prominent feature. He agreed with Dr. Luff that the cases in which necrotic changes had been observed were probably cases of rheumatoid arthritis which subsequently had developed gout. In connection with Dr. Chalmers Watson's recent observations on the changes in the blood of the gouty, he might mention that he had examined the blood of a patient suffering from chronic gout, without detecting any leucocytic abnormality. Although he agreed with Dr. Sansom that uric acid only played an incidental part in the causation of gout, Dr. Luff's results might have a considerable bearing upon the treatment of that disease, but, before offering any criticism, it would be advisable to study them carefully. As uric acid had not been shown to exert any influence on the circulatory or digestive systems, he had suggested adenin as a possible agent in the production of gout. It was very suggestive that, after four days' administration, adenin should produce deposition of sodium biurate in the kidneys of the dog, especially as they were aware that the excretion of uric acid in that animal is excessively small.

April 23rd, 1900.

ON CYSTS OF THE BREAST: THEIR RELATIVE FREQUENCY, DIAGNOSIS, AND TREATMENT.

By Thomas Bryant, F.R.C.S. Eng., M.Ch. R.U.I.

Four years ago, after having seen in consultation a very interesting series of cases of tumour of the breasts which had been considered to be examples of carcinoma, but which eventually turned out to be cases of simple cysts, I was induced to write, and subsequently to read, a paper before this Society upon "The Diagnosis and Treatment of Early Cancer and Cysts of the Breast,"* and I did so under the conviction that the frequency of cyst disease of this gland was not appreciated, that its diagnosis was not as perfect as it ought to be, and that the treatment of such cases was, consequently, uncertain and unsatisfactory. In that paper I quoted a dozen cases of simple cysts in the breast which had been suspected or pronounced to have been cancerous, and which in the majority of instances would have been treated as such, and probably by the removal of the breast, when with a correct diagnosis the cyst itself was dissected out of the affected gland or otherwise destroyed, and the breast was saved in every instance.

The experience I have had since that date, with the attention I have given to the subject since my interest in it was awakened by the compilation of that paper, has led me to review as a whole my clinical experience of breast disease during recent years, and particularly as to its bearing upon the frequency of the development of cyst disease of the gland, and I now bring before you in a condensed form the conclusions to which my investigations have brought me, of which the following are the most important:—(1) That simple cysts of the breast are far more common than they are generally believed to be; (2) that they are chiefly found in women during the same period of life as that in which cancer is met with; (3) that they are mostly quite amenable to local treatment, without the sacrifice of the breast-gland in which they are situated; and (4) that there is no reason to believe that

* 'Transactions of the Medical Society of London, 1896.'

women who have these cysts are more prone to cancer than those who have them not.

With the view of gaining some definite information as to the frequency of the presence of these cysts, I have gone to my own note-books and have carefully noted and analysed 242 consecutive cases of breast disease as they have appeared before me in private practice, for I felt convinced that it was by such means alone that a correct impression as to the comparative frequency of the affection was to be acquired. The analysis of these 242 cases comes out as follows :—163 cases were registered as solid tumours or examples of cancer or sarcoma, and 67 cases as of cystic disease. Of the 12 remaining cases, eight were examples of adenomata, two were cases of lipomata developing in the parts covering the breast, and two were cases of carcinoma in men; but these 12 cases are not now under consideration. It will thus be seen that there are 230 cases of breast disease to deal with—163 of diagnosed cancer, or sarcomatous disease, and 67 of diagnosed cystic disease, the latter, or cystic disease, bearing to the former, or cancerous disease, the relative proportion of 29·1 to 70·9 per cent. These facts suggest the conclusion that out of every four cases of tumour of the breast, more or less simulating cancer, one at least will be of a cystic or simple nature.

But it will be said that there is some fallacy in this conclusion, since, in both groups, cases which have been only diagnosed are mixed with others in which the diagnosis was proved by operation; and the force of this objection I fully admit. Consequently to get rid of this source of fallacy I have eliminated from my calculation all the cases in both groups which were simply diagnosed, and I now give an analysis of the cases of both groups which were subjected to proof by operation. By this method a like conclusion will have to be drawn, for it appears that out of the 163 cases of diagnosed cancer 126 were operated upon, and out of the 67 examples of diagnosed cyst disease 44 were operated upon, the total number of operations being 170, and these figures work out to show that of the whole number 25·8 per cent. were examples of cyst disease and 74·1 per cent. of cancerous disease, the proportion of cases being practically the same as in the larger figures, both series supporting the conclusion that out of every four cases of breast disease, more or less simulating cancer, one will prove to be an example of cyst disease. These figures consequently fully

prove the truth of the first conclusion I have formulated—"that simple cyst disease of the breast is far more common than is generally believed."

I will now proceed to show you the truth of the second conclusion, "that these cases of cyst disease of the breast are found in women at the same period of life as that in which cancer is met with," and for this purpose have analysed all the cases which have presented themselves to me, 67 in all, 23 of which were in single and 44 in married women. The analysis is as follows:—

Age.	Cases operated upon.			Cases not operated upon.			General total.
	Single.	Married.	Total.	Single.	Married.	Total.	
Under 30 years of age ..	2	1	3	1	—	1	4
Between 31 and 40 ..	4	4	8	1	2	3	11
„ 41 „ 50 ..	8	11	19	4	10	14	33
„ 51 „ 60 ..	2	11	13	—	3	3	16
Over 60	1	—	1	—	2	2	3
Total	17	27	44	6	17	23	67

By this table it appears that out of 44 cases of cysts of the breast in which the diagnosis was confirmed by operation, 11 occurred in women under 40 years of age and 33 in women over 40 years of age; and out of 23 cases diagnosed, but not proved to be examples of the same affection, four were under and 19 were over 40 years of age, the conclusion being that 52 of the whole number of 67 cases, or 77·5 per cent., took place in women over 40 years of age, the proportion in the group of cases which had been operated upon and in that in which the cases had been only diagnosed being about the same. These facts are enough for me to say that they afford ample proof of the second conclusion I have formulated, that these cases of cyst disease of the breast

are found in women at the same period of life as that in which cancer is met with.

The third conclusion I have now to support is "that these simple cysts of the breast are mostly amenable to local treatment without the sacrifice of the breast gland in which they are situated," for by a free incision through the soft tissues which cover the cysts they can be exposed and emptied, and by a simple dissection removed from the gland. Should the cyst, when exposed, present a lining membrane absolutely free from anything like papillary or other growths its excision is not, however, a necessity, for when its inner surface has been destroyed by swabbing it with either pure liquid carbolic acid, a strong solution of zinc chloride, or the tincture of iodine, there is but little probability that any growth will subsequently appear. In my own practice I have had no instance in which this took place, although when any intra-cystic growth, however small, is found in the cyst wall the dissection of the cyst out of the gland should be adopted as a rule of practice. Indeed, when any suspicious growth is found to exist in a cyst, and such a growth is of a fleshy and not of a papillary nature, the lobe of the breast involved or the whole gland should be taken away according to circumstances, the latter course being probably the safer. But such cases as these have been quite exceptional in my practice; there have been but two, and these have been classed with the solid or cancerous tumours.

The practice I have just described is the one I have for many years always adopted, and I have not yet met with a case in which it has not been permanently successful. Under these circumstances the fourth or last conclusion has its best support, for my experience tends to show "that there is no reason to believe that women who have these cysts are more prone to cancer than those who have them not," and under such circumstances there can be no reason why a breast affected with this trouble should be sacrificed. A surgeon who may have removed a breast for supposed cancer, and has found on or during its removal that the tumour was a cyst, may soothe his conscience with the thought that he has, after all, done a good thing and saved his patient from future trouble. But this conclusion will have been based on the surgeon's hopes rather than his experience. In my belief the presence of a simple cyst in the breast is no

harbinger of future evil if it be treated and removed, although if left it may without doubt become a source of mischief; and that should a cyst containing intra-cystic growths be left untreated it to a certainty will develop into a serious local affection, although when treated as I have advised the danger in the future seems to be small. I should like it, however, to be understood that I do not advocate the exploration of every cyst or of all tumours which may be cysts of the breast-gland. Small involution cysts come and go, and particularly when the whole gland has been in a coarse, irritable, and painful condition, and subsequently settles down; cysts of the size of a nut, and even of a walnut, occasionally disappear under an improved condition of health, and particularly of the pelvic organs when they have been disturbed. In the paper to which I have already drawn the attention of this Society I recorded two cases in which this happy event took place, and it is doubtless such cases as these which, when they fall into the hands of quacks and undergo quack treatment, are regarded as examples of cancer and published as cancer cures. The cases upon which this paper is based were all examples of tumours as large as, or larger than, an unshelled walnut; each one had existed for some months, and was the seat of local uneasiness, if not of pain. In the minds of their possessors they had excited fears, as well as in the minds of the medical men who had been consulted about them; in fact, all the cases were examples of breast disease which should justly claim the surgeon's close attention.

In some of the instances, the lump in the breast existed alone, without any collateral symptom to help a diagnosis. In others, a discharge from the nipple of some kind was present. In a few the patients reported that the tumour varied in size as well as in hardness at different times, and these symptoms should always lead the surgeon to suspect that the tumour might be a cyst. In exceptional cases there was some traction upon or retraction of the nipple, this condition tending to lead the surgeon towards a diagnosis of cancer, although it is quite consistent with the presence of a cyst. In none of the cases was there any dimpling or infiltration of the skin over the tumour as in cancer, although in several the skin was involved by stretching, and this in exceptional instances may be carried to the extent of its rupture. I have seen examples of this with rupture of the cyst and the discharge of its contents externally.

A serous cyst is sometimes met with in a breast which is, as a whole, undergoing involution changes. The gland thus affected feels on manipulation cordy, or, as I prefer to describe it, as a "coarse" gland. The involution cysts, when exposed by the incision into the breast, appear as greenish or black spots of about the size of a hempseed in the substance of the gland; at times they appear in strings. Their appearance is well illustrated in Fig. 5, Plate VIII, p. 317, of my work on 'Diseases of the Breast.'

The presence of these small cysts when associated with a simple serous cyst which calls for treatment does not require the removal of the breast-gland, although hitherto surgeons have deemed it right to adopt this practice. I have followed their example in past years, but extended experience has confirmed me in the belief that it is not necessary, for the affected gland after the removal of the cyst has, in all my cases so affected, subsequently settled down without any future troubles manifesting themselves. In fact, the "coarse" breast, associated with a cyst, has settled down after operation just as well as the "coarse" breasts the subjects of involution without serous cysts have done, and generally do by time and treatment. The removal of the one is no more necessary than the removal of the other. When, however, intra-cystic growths are found to be present in the cyst which is being dealt with, and the breast generally is full of small involution cysts, the extirpation of the whole gland is a right measure to adopt.

It is foreign to my purpose to enter into the pathology of this affection, although, since it occurs mostly in women over 40 years of age, it seems probable that the cysts are originally involution cysts, and as it is very commonly associated with the occasional discharge of a more or less clear fluid from the nipple, it seems justifiable to conclude that the ducts of the gland are in connection with the cyst, if a dilated duct is not its origin. Some of these cysts, and, roughly calculated, less than half of those explored, will be found to contain small papillary growths attached to their walls. In one case these will appear like small red sessile buds of the size of mustard seeds, in another they will be pedunculated, whilst in exceptional cases the cyst will be found to contain solid growths. It seems probable, however, that all large cysts of the breast, if left untreated, will sooner or later

become the seat of some proliferating intra-cystic growth, which will be papillomatous, sarcomatous, or carcinomatous in its nature, according to the proclivity of the tissue to form, or of the individual to develop, either special variety; in my book on 'Diseases of the Breast' I have quoted a series of cases to illustrate these points. The cases which I found in practice to contain solid growths have not been included in this communication, as they have been grouped with the solid sarcomatous or cancerous tumours.

Why these cases of cyst diseases of the breast are so frequently overlooked and left to be discovered by accident is a question which may be difficult to answer. I believe it is so from two main causes, the first being the strong belief that such cases are rare, and are not, therefore, to be expected, and the second being the fact that these cases are met with in women at the same period of life as that in which cancer is known to appear. For it is true that the professional as well as the public mind is so firmly impressed with the idea that any tumour, not inflammatory, appearing in the breast of a woman over 40 years of age is probably cancerous, as to forbid any other explanation of its nature being seriously entertained.

The facts I have laid before you clearly indicate that this opinion is not a correct one; for if we take all the cases of tumour of the breast together—excluding inflammatory affections but including the cases of carcinoma—presenting symptoms of so marked a character as to forbid any error in diagnosis being made, it has been shown in my analysis of cases that in one out of every four the tumour will be cystic, and not cancerous; whereas if we eliminate all such well-marked examples of cancer from our consideration in which an error in diagnosis can hardly occur, and apply our argument to those alone of a doubtful nature in which there are no collateral symptoms of cancer to support a diagnosis, it does not appear to be wrong to conclude that in every two cases of this kind one will be cystic. I must confess that this conclusion came upon me somewhat as a surprise, but the facts I have laid before you clearly suggest no other, nor will they bear any other interpretation. I do hope, therefore, that the professional mind will accept the truth as I now present it, and in any individual case of breast tumour not clearly cancerous supply the argument of probability, and, instead of

assuming at once that the disease is probably cancerous, remember that the logic of facts suggests that the case is as likely to be one of a simple cystic nature as of a malignant kind. In my practice it is certain that this has been the case. To admit that my experience in this matter is exceptional is what I cannot do; indeed, I know of nothing in my practice which differs from that of other consultants, although it is just possible that I may have been consulted more in difficult cases of breast disease than in those of cancer that are fairly evident, and, if so, I may therefore have had a larger proportion of cystic breast cases to deal with than other men. But I do not believe this to have been the case, nor do I think that such an explanation need alter materially the conclusions I have formulated, nor make them less worthy of your attention.

With respect to the diagnosis of these cases there may be a difficulty, and particularly in the case of a middle-aged woman with a hard, slow-growing lump in her breast, and in whose history there is clearly an absence of the influence of lactation and of any inflammatory breast condition, and the condition of the nipple and soft parts covering the lump are normal. In such a case as this the surgeon should put to himself the question, Is it one of cyst, or is it one of early cancer? for the chances of either have been shown to be equal. To decide, or rather to help towards the diagnosis, a careful local examination should be made, with the patient placed flat upon a couch and the breast-gland made to rest upon the ribs, when, should the surgeon's sense of touch be keen enough to detect an elasticity in the swelling, or even fluctuation, the diagnosis of a cyst may reasonably be hazarded, although what the nature of the contents of the cyst may be cannot be otherwise than obscure. If a clear serous fluid can be made to flow from the nipple by manipulation of or pressure upon the tumour, the probabilities of the cyst being a simple serous cyst are much enhanced. If the fluid be brown or blood-stained, the existence of an intra-cystic growth of some kind may be suspected; and if the discharge be more like pure blood, the presence of a soft solid growth, sarcomatous or carcinomatous, should be feared. When, however, there is no nipple discharge, as is often the case, and the other conditions are the same, the chances against the swelling being caused by a cyst are not lessened, although the view of the supposed cystic

enlargement of the breast being due to the presence of some soft cancerous growth would be encouraged.

To make the diagnosis certain an exploratory incision should be undertaken, when, if the lump be due to a cyst, it should be dealt with as already described; and should the tumour be due to a cancerous infiltration or sarcomatous disease, the breast-gland should be removed; the operation being in these circumstances undertaken at the period of the tumour's growth after which the most favourable result may fairly be expected. To allow a cyst to enlarge and persist for months, in order to make its diagnosis more certain, is a timid and dangerous practice; and to allow a tumour which may be cancerous to drift until collateral symptoms have made their appearance, so as to make its diagnosis certain, is neglectful and culpable. For, to the patient, delay in removing the local disease, whether cystic or solid, is fraught with danger, whereas by early interference nothing but good can be achieved.

Dr. G. Steele Perkins agreed that the difficulty of diagnosing a simple tumour of the breast from carcinoma was often very great, and he instanced two cases in his own experience which tended to support this view. The first case dated back 20 years, when he was a student at Guy's Hospital. The patient was about to be operated upon by the late Mr. Cooper Forster, who described the case to his class as one of mammary cancer, with all the typical signs and symptoms. When, however, he incised the breast pus escaped; whereupon, with characteristic honesty, he directed the attention of the students to the lessons to be learned from his error in diagnosis. The second case was one which occurred in his own private practice 17 years ago. A lady about 38 years of age, without children, came to him with a lump in the breast. After careful examination he came to the conclusion that it was a simple tumour which could be relieved by merely letting out its contents. A second opinion, however, was desired, and Mr. Jonathan Hutchinson was called in. He, without hesitation, pronounced it to be cancer, and advised immediate operation, but, on incision, pus flowed out. These two instances, he remarked, occurring in the experience of two very able men, showed how easy it was to make a mistake in diagnosing these cases. The second patient was a highly neurotic woman, with a history of injury nine months before, which had been followed by much pain, especially after she had heard of a similar injury, in the case of a friend, producing cancer. The nipple was unquestionably retracted, but the tumour was hard and only about the size of a walnut.

Dr. Campbell Pope observed that, in general practice, these cases were seen in their early stage, and were often difficult to diagnose. Undoubtedly there was frequently a history of injury in cases which ultimately turned out to be carcinoma, a point to which the author had not referred. The author, moreover, had not said much of the significance of enlarged glands in establishing the diagnosis of carcinoma. Enlarged

glands might be present when there was inflammatory mischief, and thus tend to confuse the diagnosis. Personally, he attached most importance to involvement and dimpling of the skin, especially the latter, which was often the first thing to direct attention to the fact that there was serious mischief in the breast. He agreed with the author as to the probable proportion of innocent tumours in the breast, and would be disposed to consider that it was even greater than 50 per cent.

Mr. Bryant, in reply, pointed out that his paper did not profess to deal with cancer or abscesses of the breast, but only with cysts of that organ. He remarked that the late Mr. Forster was one who avowedly ignored the history, preferring to base his conclusions on what he himself saw, but this method was very apt to lead one wrong. The history of injury in Dr. Perkins's case might well have raised a doubt in the surgeon's mind as to the possibility of its not being cancer. He held that, in the majority of these cases, if one utilised the information afforded by the history, they might, as a rule, arrive at a correct diagnosis. He had not referred to the significance of enlarged glands in establishing the diagnosis of cancer, because he did not believe that fact had any significance, inasmuch as enlarged glands might be present independently of carcinoma; and, moreover, he was only dealing with doubtful cases. He admitted that the figures he had brought together had surprised him, the more so as they were based on consecutive cases. Cysts of the breast were looked upon as somewhat uncommon, but his figures showed the contrary.

TWENTY-FOUR YEARS' DEATH CERTIFICATES IN GENERAL PRACTICE.

By H. Campbell Pope, M.D. Lond., F.R.C.S. Eng.

The busy general practitioner has little time to take note of what he is doing, and he sees no such results growing round his work as is the case with those who practise in a less extended sphere of human pathology, and who draw large deductions and, may I say, larger emoluments from a minute study of isolated portions of the fields of medicine and surgery.

But it occurred to me that though our hospitals continually publish the results of their work, the far greater work and gathered experience of thousands of practitioners die with the men themselves, and little is known of what cases pass through their hands or what deductions they may have drawn from their experience. It then struck me that the counterfoils of my death certificate books might be of some service in showing, possibly, what diseases were prevalent in that portion of the west of London in which I practise, and also might throw some light upon hereditary transmission of disease. But I approached the

subject with a perfectly open mind and determined to tabulate the certificates and see if any useful results would flow from their inspection. I found that a great variety of diseases entered into the composition of such a tabular form, and that the incidence of disease in the locality was not such as I judged it to be previous to starting this investigation. The total number of deaths during the period was practically about 600. I have tabulated 595 of these, but a considerable number of inquests—in fact, all but one case, a death from chloroform inhalation—have been omitted.

The first section I have tabulated is malignant diseases, which shows 51 deaths out of 600, equal to 1 in 12, which quite surprised me, as I had no idea that there was anything like so great a proportion. Of these, 14 were of the liver, 12 of the uterus, three of the intestines, five of the breast, two of the rectum, one each of the stomach, pylorus and rectum, pancreas, kidney, inguinal glands, penis, tongue, jaw, cheek, tonsil, and foot; two of the brain, and three sarcomas, two of the kidney and one of the mediastinal glands.

As regards the liver cases there is no doubt that alcohol had a good deal to do with them; several of them were large drinkers, especially of whisky, and in some of the other cases gall-stones were present. In one of the cases kindly recommended to me by my friend Mr. Keetley, there was an immense gall-stone impacted in the common bile duct, which I have here. In all these cases, I should look upon the alcohol and the gall-stones as constituting the primary irritants which seem so often to precede cancerous developments.

As regards the uterine cases, only one was operated on, and did well as regards the cancer, which was of the fundus, but the ureter was wounded in the operation, and pyelitis resulted, ultimately attacking *both* kidneys. Of the breast cases, secondary developments in the liver were observed twice, in the brain once, in the œsophagus and stomach twice, and in the lungs once. The case of cancer of the kidney occurred in the person of a patient affected with hæmophilia, and was fatal from hæmorrhage. The case in the inguinal glands was primary, and death occurred from ulceration into the large vessels. The prominent symptom in one of the brain cases (glio-sarcoma) was pain of the most intense character. The two cases of sarcoma of the kidney were both interesting: one, a little girl, had secondary sarcoma of the liver,

and was in a cataleptic state for a long time before death. In whatever position the limbs were placed, even the fingers, there they would remain. The other case was a barrister who once passed an oxalate of lime calculus; he then suffered with renal abscess, which burst periodically, many times, through his lungs, and the contents were expectorated; ultimately, sarcoma of the kidney and the glands right up to the cervical region supervened. The case of mediastinal sarcoma was seen by our President; the patient died of suffocation, a large quantity of soft material was expectorated, and the lungs were flooded by it, causing the patient's death. These are, of course, all deaths from cancer. Of other cancer cases which have come under my notice, I have found those of the breast most amenable to operative treatment.

Diseases of the Kidneys.—"Chronic Bright" is answerable for seven out of the 14 deaths. These cases mostly died of heart or lung complication. Over and above these ordinary cases was one of "chronic Bright," in which innumerable calculi were passed from the bladder by a woman; 20 or even 30 at a time were quite common, and this went on for years; the calculi were of the size of lentils or small peas, and mostly round. Chronic Bright, with attacks of much albumen, is not so formidable as the constant passage of small amounts. Three other cases ended in uræmia; one, a very remarkable case of a most distinguished musician, was complicated by diabetes and eczema of the legs. The discharge of water from the eczematous legs was enormous, and doubtless to some extent purified the blood. This gentleman was a great worker, and to the last persisted in his duties, working 10 or 12 hours a day at composing or musical editing, although he could only move with difficulty from his chair. Another good and instructive case was that of an actor, who, after a long railway journey and sea trip to the Isle of Man, arrived heated at the theatre, which was very draughty. He played his part, but found he could not pass water on his return home to his lodgings. In the morning the same occurred, and he consulted a doctor, who passed a catheter with no result. He then returned to London, and was under my care for several days, and was seen in consultation by Dr. Stephen Mackenzie, but our efforts to induce the flow of urine were unavailing. A case of stone in the bladder was of some interest in a little girl of 3; this I removed, and she went on well until she was about 16, when she showed signs

of stone in the kidney. She was operated on in University College Hospital by Mr. Marcus Beck, and a fistulous opening remained in the back. She had several attacks of uræmia, and ultimately died in one, nine years after, at the age of 25.

Diseases of the Lungs.—Of these, which are 170 in number and form by far the largest section of my table, the cases of bronchitis are most numerous, the acute cases being 30 and the chronic cases 23. These are independent of the cases of whooping cough, which I have placed in a separate sub-section. The great majority of the acute cases were in children, and some few in old people, without pre-existing chronic bronchitis. Of unusual causes of bronchitis, one was due to the fumes of vinegar. A gentleman received an appointment in a vinegar factory, and his first experience of the fumes was fatal to him. Two of the cases of chronic bronchitis presented this peculiarity, that the lungs would become suddenly œdematous and put the patient in danger for several days. In one of these two cases the man's hair turned grey in each attack, and resumed its pigmentation in three or four weeks after the attack was over.

Phthisis accounted for 42 deaths; this somewhat surprised me, as we have been accustomed to think of Shepherd's Bush as a place where phthisis is comparatively unknown. It has been more prevalent during the last 10 years, since influenza made its appearance, but cases due to influenza I have relegated to an influenza section. It would take far too long to go into points connected with these cases of phthisis; I would only refer to three in which death occurred from pneumothorax. Two of these were in young girls; the bursting occurred in a paroxysm of cough, and both had subcutaneous emphysema. The third case was a man who was going to be operated on for fistula. While inhaling chloroform before the operation he suddenly died, and it was found, *post mortem*, that a sub-pleural cavity in the apex of one of his lungs had burst. Of the other respiratory cases, 12 were broncho-pneumonia, four catarrhal croup, three laryngismus stridulus, two congenital asthma, one of plastic bronchitis in a young domestic servant who slept over a stable, one of tonsilitis. It may be of interest to note the complications which caused death in 24 cases of whooping cough. In one, vomiting; in six, bronchitis; in five, convulsions; in seven, pneumonia; in one, pleurisy; in one, harelip and cleft palate, which caused an

extraordinary embarrassment of breathing; in one, meningitis; and in one, broncho-pneumonia. Pneumonia was the commonest cause of death.

Diseases of the Heart and Circulation were 46 in number. Four of these were congenital. These died at the following ages: seven years, 38 hours, 21 years, and 22 years; all were cases of morbus ceruleus and had right-side heart disease. One case of aortic aneurysm. One case was complicated with hæmophilia and was husband to the lady who died of kidney cancer and hæmophilia; their family, both males and females, have hæmophilia, and one daughter died from hæmorrhage during typhoid fever, which will be mentioned later. I have had several exciting times with them after dental extractions and quinsies, to which they are subject. One case was complicated by obesity, considerably over 20 stones. One case had moist gangrene of the whole leg. One case had a large gangrenous ulcer of the leg, which was excised by my friend Mr. Keetley, and did well for a time. One case of phlebitis of the arms and legs in a stout lady, died suddenly 10 minutes after my visiting her, when she was sitting up enjoying her lunch. One case of heart disease had extreme pain; Dr. Mitchell Bruce saw the case with me several times, but we could not exactly determine its nature, and no *post-mortem* examination was allowed. The man would shout with agony, but there was not extreme dyspnœa. One case of adherent pericardium simulated aneurysm, and the patient died momentarily from rupture, a huge quantity of blood pouring from the mouth. On the whole, organic heart disease would seem to be a favourable sort of disease to have, the average of the 46 deaths being $57\frac{1}{2}$ years, even including the young congenital cases. I think this is perhaps too favourable a view, as inquest cases are excluded from this table. Many such cases are heart cases, and very often in those who are not aware of any heart trouble being the matter with them. Those who are aware of their infirmity will sometimes take great pains not to over-exert or flurry themselves, and consequently live on to old age. Two of the cases of mitral disease were brother and sister aged about 70. The late Sir Andrew Clark said to me, "I do not give a despondent opinion in cases of mitral murmur."

Scarlatina accounts for nine deaths. Three of these were scarlatina anginosa with a gangrenous condition of the cervical

glands. These cases begin with an acrid discharge from the nose, and I have a strong suspicion that they are cases of post-nasal diphtheria with scarlet fever, and I think it would be well in such cases to use the diphtheria antitoxin. One other case was evidently associated with diphtheria. In one of the cases the finger could, after death, be passed right through the neck, the destruction having been complete on both sides. In this same house a boy of another family several years afterwards died of diphtheria. The complications in the remaining five cases were one scarlatina maligna, one general eczema, two of nephritis, and one of parturition.

Typhoid.—Four deaths occurred from typhoid. One from convulsions, one with whooping cough as well, one with tubercular meningitis, and one, the hæmophilia case mentioned above. This young lady had nursed her mother for several months with cancer of the kidney, and was much run down in health, when she was stricken with typhoid and died of hæmorrhage.

Diphtheria.—It was seven years after I started practice before I saw a case of diphtheria. Then in an evil hour the London School Board secured a cheap site, where a marshy pond existed and had existed for years. This they somehow dried and built a Board School over it, which fell down before the roof was finished. It was situated so low, below the level of the sewer, that it was impossible to drain the school, and a great collection of impurities took place in the unhealthy foundations. Diphtheria soon started, and there it has remained ever since; nearly all our cases of diphtheria come from the district supplied by this school. The first case was a very severe one, and I urged the father to leave the house, as I felt sure that drainage had something to do with the matter, being in such close proximity to this unhealthy place. He did not agree with me, and the following year he lost two more of his children, one with general paralysis after the attack. Thus, of the five fatal cases three were in one family. Numerous deaths of children from that school have occurred, many in the fever hospital.

Measles.—Of the 18 measles cases, one died of abscess and pyæmia, one of convulsions, nine of pneumonia, one of pneumonia and apoplexy—this was an adult woman of 34, one of hæmorrhagic measles—a child on whom I had previously performed inguinal colotomy for congenital deficiency of the rectum. This case is

reported in the 'Proceedings of the West London Medico-Chirurgical Society,' vol. i. One case also, an adult lady of 29, died of acute mania with measles; she was addicted to alcohol. The other cases had bronchitis (two of them) and one also had diarrhœa; only one case was uncomplicated. The point of note is that the two adult cases died of cerebral complications.

Diabetes (eight).—Two died of coma, one of congestion of the liver and jaundice, one chronic case succumbed to influenza and pneumonia, and one was exhausted by a large carbuncle on the abdomen of the size of a cheese plate.

Rheumatism.—Of these complaints there were 11 deaths. Two had chronic bronchitis and one pericarditis in addition, one had hyperpyrexia. This was a case to remark. A gentleman went for a bicycle ride in an east wind; he sweated profusely. To his surprise when he got home the sweating continued and he felt ill and sent for me. I found him sweating profusely, temperature varying from 103° to 105°; this went on for some days, when Dr. Wilks saw him with me, but the sweating and fever persisted till death. One case had phthisis, three had heart complications, and one tonsilitis, hæmoptysis, and hæmatemesis; one was a case of rheumatoid arthritis.

Senile Decay.—Eight uncomplicated cases; the oldest was 95. One case was Raynaud's disease, nearly all the toes fell off and the tip of the nose.

Influenza.—I was pleasantly affected in studying these certificates to find how few deaths there were considering the enormous number of patients treated. Influenza accounted for 18 deaths, and I think 5,000 would be a modest estimate of the cases medically treated in the practice. Of these cases, one died of cardiac failure, two of pneumonia (there was also the gentleman named under diabetes), one of phthisis, one of pneumonia and phthisis, one of diarrhœa, vomiting, and coma vigil, one of abscess and erysipelas, one of cystitis and septicæmia, and one of pleuro-pneumonia. There was no death from influenza *per se*, and I have little doubt that few of these patients would have died had they taken to bed on the first symptoms appearing. That is the essence of influenza treatment, and I find salicylate of soda and citrate of potash in the early stage, and quinine and nux vomica in the convalescent stage, sufficient for nearly all cases. Phenacetin is most useful for the pain. I have digressed somewhat here, for so

many fatal results have occurred through indiscretions in regard to chills during the early stages of the disease, that I think the point cannot be too strongly insisted on. In 1889 I called attention to this point in the 'British Medical Journal.'

Of *General Tuberculosis* there were two cases; one died of diarrhœa and exhaustion, and both were under one year.

The two *Tetanus* cases were both tetanus neonatorum; the old plan of carbonising the rag used for enwrapping the navel cord doubtless sterilised the linen and had something to recommend it. Experience showed that cords so dressed did not stink.

Hæmophilia.—Two cases are noted under this heading, although three others which have been already alluded to died from intercurrence of other diseases. One of the two was an infant who died of umbilical hæmorrhage. The other was a case of great interest and was under observation for many years. His chief trouble was hæmorrhage into the knee-joints which frequently occurred. His fatal attack was in the brain.

The Liver cases were 14 in number. One was a malarial case, one a case of atrophy, three of gall-stones, and the others cirrhosis, probably all alcoholic cases.

Gout (nine).—These were all chronic cases. Three died of bronchitis; one was very interesting in this way, at the age of 76 she had whooping cough, which resulted in bradycardia, with a pulse averaging 40, this rate persisted until she died in January last at the age of 87. Another case had rheumatic fever with mitral disease and dropsy, which is interesting as showing that the two poisons may co-exist, I have reason to believe that this is not infrequently the case. A third case had gouty kidney, and at one period of his illness was afflicted with ulceration of the inguinal glands, this was very extensive and deep. Mr. Thomas Bryant saw the case with me, and we agreed that probably the cause of it was the gouty poison. He escaped ulceration into the deep vessels, and lived for several years after, ultimately dying of hemiplegia. Of the remaining two cases one was afflicted with senile decay, the other with fatty heart.

Anæmia was answerable for three deaths. Two were of the pernicious variety.

Purpura (one).—The patient, a girl of 16, died from exhaustion consequent on hæmorrhage from the gums and hæmatemesis.

Debility and Malnutrition.—Under this heading 62 deaths are noted. Of congenital debility there were 26 single infants, four pairs of twins, and one set of triplets; premature birth accounted for 14; rickets and convulsions for five; syphilis and convulsions for three; marasmus and convulsions for two; and rickets and purpura for one. There is here a considerable waste of infant life, and I cannot doubt that many cases of congenital debility and premature birth result from the medicines which are used to procure abortion, and the means employed to prevent conception. These practices are widespread amongst all classes, and it is much to be desired that the authorities would compel the advertising press to close its columns to notices of nostrums and implements devoted to these ends. The Medical Defence Union has done much in this direction, but without the support of those in authority it cannot do much more. The matter is one, in every sense, of vital importance, for the future of the British Empire is greatly concerned in the prompt and ample supply of healthy British children.

Diseases of the Brain and Nervous System.—These diseases caused 69 deaths; 17 were hemiplegia; 13 tubercular meningitis, which appears to me a somewhat large proportion; seven of simple meningitis; four of convulsions (cause unknown); four of cerebral softening; two of chronic spinal meningitis, these are interesting cases and have been fully described by Dr. Barlow; one of chronic imbecility; two of chronic alcoholism—as regards alcoholism many cases under liver and kidney were alcoholic, these two were uncomplicated alcoholism; two of spina bifida; two of meningocele; six of dentition and convulsions; one of sclerosis of the brain; one of cerebral tumour; and one of paralysis agitans. The case of cerebral tumour was a little girl, she was quite blind and suffered much pain; the case was seen by Dr. James Taylor with me. The case of sclerosis of the brain was a singularly pathetic one—the causation was alcoholic. I had occasion to speak to the lady on the cause of her disease, and she seemed to pay attention to my words, a very unusual fact in an alcoholic female; at any rate, she dropped her stimulants, and almost immediately afterwards her husband failed in business, and this seemed to complete the reformation in her habits. She buckled to and kept the home together in a most exemplary manner, and was the means of restoring the position of her family, and died shortly after her

husband gained another situation, and fortune was again smiling upon them. In a large experience of such cases she was the only female I ever saw reform from moral motives.

Extra-Uterine Gestation is perhaps badly described. The case was one of impregnation in an undeveloped horn of a uterus bicornis unicollis, and is fully noted in vol. xxviii of the Obstetrical Society's 'Transactions,' p. 70.

Ovarian and Uterine Cases, exclusive of cancer, only number two deaths, one of ovarian dropsy, and one of peritonitis occurring in a case of chronic endometritis.

Parturition.—The deaths connected with child-bed are nominally eight, but half of them were connected with other serious diseases. In one case advanced phthisis, in another tubercular peritonitis and suppuration brought on premature labour, this was operated on by Mr. Keetley. In a third case I was called to a woman who had been attended by a midwife, who ran away from the case. I found the woman moribund from heart disease and cardiac asthma and bleeding profusely from placenta prævia. I delivered by turning as rapidly as possible, but she only survived one hour. In another, advanced heart disease caused fatal post-partum hæmorrhage. The remaining four cases were more legitimate puerperal deaths: one from septicæmia and clots in the heart, one from uræmic convulsions, one from violent ante-partum hæmorrhage, and one from peritonitis. Total number of parturition cases, 1,700.

Hernia.—One case only ended fatally, an old inguinal hernia became obstructed in a man of 83 with fatal result.

Erysipelas and Septic Diseases.—Three cases of otitis died—one with erysipelas, one with meningitis, and one with septic bronchopneumonia. Three ordinary cases of erysipelas—one case of septicæmia from umbilical putridity, one of senile decay, complicated by abscesses, and one, a case of some interest, of severe scalp wound, prolonged suppuration ending in albuminoid disease of the kidneys and liver. This gentleman survived the injury a year and a day, thus exactly escaping an inquest by the expiration of the legal limit of time. This is a point not always understood by members of the profession.

Diseases of the Digestive System accounted for the decease of 42 persons; of these 29 were diarrhœa and gastro-intestinal catarrh, four of obstruction of the bowels, which possibly in these

later days might have been cases of appendicitis and operated on, four of gastritis and jaundice, one of glossitis and asphyxia, one of dysentery, and three of peritonitis. With regard to the 29 cases of diarrhœa, most of these were infants. Here I cannot refrain from noting the increasing prevalence of the custom of avoidance of suckling. Two principal reasons may be adduced for this: one, the necessity of earning a livelihood amongst certain classes, the infants being put out to nurse; the other, a dislike of trouble and an unfounded fear of losing their figure amongst better-to-do people; some few appear to have no milk supply. Whatever the cause of the loss of maternal nourishment may be, the result to infant life is disastrous, notwithstanding the glowing advertisements of various artificial foods. If only one month of natural feeding can be gained it is a great advantage.

Lymphadenoma.—One case occurred—death at 13 years of age.

Bladder Diseases were answerable for five deaths; one from tubercular ulcer and general tuberculosis—this was in the early days of the cystoscope, and the ulcer was very clearly shown by Mr. Hurry Fenwick, one case of cystitis and pyelitis, two of enlarged prostate and cystitis, one of calculus and suprapubic cystotomy; this case was also operated on by Mr. Keetley.

Inflammation of the Lower Jaw killed the last patient on my list at the age of five weeks.

Mr. President and Gentlemen, I fear I have wearied you by this long recital, but nevertheless I think if this or some other plan could be devised by which general practitioners could save for collation the results of their experiences a great gain would be derived. Especially might this be done with advantage in localities, say by societies such as now exist in most districts of London. When about 10 years ago I had the honour of occupying the chair of the West London Medico-Chirurgical Society, I devised a case-taking form which was adopted by the Society, its object being to collect information as to the effects of environment and heredity in determining the structure and present condition of any individual, on the lines laid down by Dr. Harry Campbell in his remarkable work on the 'Causation of Disease.' I still think much might be done in this direction by general practitioners.

You may ask, And what is the conclusion of the whole matter as regards the facts just brought forward? To this I would

reply that the district I practice in would seem to be more afflicted with respiratory diseases than anything else; that infectious diseases are not very prevalent, mainly due, as I believe, to the excellent arrangements of our local medical officer of health, Dr. Collier; that cancer appears much more frequently than I had thought possible; and that influenza is not a remarkably dangerous disease if taken in time, and that there is a lamentable preventable wastage of young human life.

In conclusion, I have to express my indebtedness to my partner, Dr. Hill-Wilson, to my assistant, Dr. Viliesid, and to other gentlemen who have worked with me, for a good many of the material facts set down; and I thank you all for the patient hearing which has been given to a rather dry paper.

Twenty-four Years' Death Certificates, 1876–1900.

General Summary.

Carcinoma 51
Liver, 14; uterus, 12; intestine, 3; breast, 5; rectum, 2; stomach, 1; pylorus and rectum, 1; pancreas, 1; kidney, 1; inguinal glands, 1; penis, 1; tongue, 1; jaw, 1; cheek, 1; brain, 2; foot, 1; tonsil, 1; sarcoma, 3 (kidney, 2; mediastinal glands, 1).

Kidney 14
Chronic Bright, 7; ditto c̄ calculi, 1; ditto c̄ uræmia, 3; ditto c̄ diabetes and eczema, 1; acute N. c̄ suppress., 1; stone in bladder and kidney c̄ nephritis, 1.

Lungs 170
Bronchitis, acute, 30; ditto chronic, 23; phthisis, 42; ditto c̄ pneumothorax, 3; broncho-pneumonia, 12; croup (catarrhal), 4; laryngismus S., 3; asthma, congenital, 2; plastic bronchitis, 1; tonsilitis, 1; pertussis, 24 (of these — 1 vomiting, 6 bronchitis, 5 convulsions, 7 pneumonia, 1 pleurisy, 1 hare lip, 1 meningitis, 1 broncho-pneumonia).

Heart and Circulation .. 46
Congenital, 4; aneurysm of aorta, 1; hæmophilia, 1; obesity, 1; gangrene of leg, 1; gangrenous ulcer of leg, 1; phlebitis of arms and legs (sudden), 1; extreme pain, 1; adherent pericardium (death from hæmorrhage), 1. Average age of these cases, 57½ years.

Scarlatina 9
Anginosa, 3; maligna, 1; eczema, 1; nephritis, 2; parturition, 1; diphtheria, 1.

Typhoid 4
Convulsions, 1; pertussis, 1; tubercular meningitis, 1; hæmophilia, 1.

Diphtheria (3 in one family) 5
Membranous croup, 1; paralysis, 1; ordinary, 3.

Measles 18
Abscess and pyæmia, 1; convulsions, 1; pneumonia, 9; pneumonia and apoplexy, 1; hæmorrhagic, 1; acute mania, 1; bronchitis and diarrhœa, 1; bronchitis, 2; ordinary, 1.

RHEUMATISM 11
Chronic bronchitis, 2; ditto c̄ pericarditis, 1; acute ditto c̄ pneumonia, 1; hyperpyrexia, 1; phthisis, 1; rheumatoid arthritis, 1; heart complications, 3; tonsilitis c̄ hæmoptysis and hæmatemesis, 1.

DIABETES 8
Coma, 2; carbuncle, 1; influenza and pneumonia, 1; congestion of liver and jaundice, 1.

SENILE DECAY 8
Includes one case of Raynaud's disease.

INFLUENZA 18
Cardiac failure, 1; pneumonia, 11; phthisis, 1; pneumonia and phthisis, 1; diarrhœa, vomiting, and coma vigil, 1; abscess and erysipelas, 1; cystitis and septicæmia, 1; pleuro-pneumonia, 1.

GENERAL TUBERCULOSIS .. 2

TETANUS 2
Both neonatorum.

HÆMOPHILIA 2
Hæmorrhage from navel (12 days), cerebral hæmorrhage (32 years).

LIVER 14
c̄ Malaria, 1; atrophy, 1.

GOUT 9
Bronchitis, slow pulse, 3; rheumatic fever, mitral dropsy, 1; Bright, ulcerated inguinal glands, 1; senile decay, 1; fatty heart, 1.

ANÆMIA 3
Pernicious, 2; exhaustion, 1.

PURPURA 1
Hæmorrhage from gums and hæmatemesis.

DEBILITY AND MALNUTRITION 62
Congenital debility, 26; ditto, four sets of twins, 8; ditto, one set of triplets, 3; premature birth, 14; rickets and convulsions, 5; syphilis, 3; marasmus and convulsions, 2; rickets and purpura, 1.

BRAIN AND NERVOUS SYSTEM 69
Hemiplegia, 17; tubercular meningitis, 13; meningitis, 7; convulsions, 4; softening, 4; chronic spinal meningitis, 2; chronic imbecility, 1; chronic alcoholism, 3; spina bifida, 2; dentition and convulsions, 6; meningocele, 2; sclerosis, 1; cerebral tumour, 1; paralysis agitans, 1.

EXTRA-UTERINE GESTATION .. 1
'Obs. Trans.,' vol. xxviii, p. 70.

OVARIAN AND UTERINE .. 2
Ovarian dropsy, 1; chronic endometritis, peritonitis, 1.

PARTURITION 8
Septicæmia and clots in the heart, 1; uræmic, 1; antepartum hæmorrhage, 1; phthisis, 1; peritonitis, 1; morbus cordis, P.P.H., 1; tubercular peritonitis, premature, 1; placenta præv. c̄ morbus cordis, 1.

HERNIA 1
Inguinal obstructed.

DIGESTIVE SYSTEM 42
Diarrhœa and gastro-intestinal catarrh, 29; obstruction of bowels, 4; gastritis and jaundice, 4; glossitis and asphyxia, 1; dysentery, 1; peritonitis, 3.

ERYSIPELAS AND SEPTIC .. 9
Otitis (erysipelas, meningitis, broncho-pneumonia), 3; ordinary, 3; septicæmia (umbilical), 1; senile decay c̄ abscesses, 1; injury to head, prolonged suppuration, albuminoid liver and kidneys, 1.

LYMPHADENOMA 1

BLADDER DISEASES 5
Tubercular ulcer and general tuberculosis, 1; cystitis and pyelitis, 1; enlarged prostate and cystitis, 2; calculus and suprapubic cystotomy, 1.

INFLAMMATION OF LOWER JAW 1
Infant.

TOTAL 595

May 14*th*, 1900.

ON EXCISION OF THE GASSERIAN GANGLION FOR EPILEPTIFORM NEURALGIA OF THE FIFTH NERVE.

By J. Hutchinson, Jun., F.R.C.S.

In the 'British Medical Journal' of November 5th, 1898, in a paper on this subject, I recorded in full the first case in which I had the opportunity of performing this operation. The patient had previously undergone many operations on the branches of the fifth nerve, including the removal of Meckel's ganglion, but invariably after an interval the pain had returned with increasing severity. His life had become a complete burden to him, and the use of large doses of morphia, which alone gave him any relief, was attended by the usual depressing effects. Since the excision of the greater part of the Gasserian ganglion with the roots of the second and third divisions of the fifth nerve he has been completely cured of his pain. I have seen him many times during the last two and a half years, and the only symptom he has complained of in the face has been occasional and slight spasm of the muscles on that side. This spasm is no doubt a relic of the violent contractions brought on by the neuralgia, which used to occur almost every moment.

He returned to work soon after the operation, having previously been incapacitated for long, and has gained in weight materially. This latter result has been still more marked in a second case in which I excised the ganglion, the patient being an elderly woman who was quite worn out by her sufferings. When she returned to report herself some three or four months after the operation I hardly recognised her, so much had she improved in appearance. Although the masticatory muscles (the temporal, masseter, and pterygoids) must of necessity atrophy as a result of excision of the ganglion, it is remarkable how little trouble in eating is caused thereby, and since taking food no longer brings on attacks of pain the patients are really much better able to take their meals than before. At the same time excision of the Gasserian ganglion on both sides is obviously out of the question, owing to the paralysis of the jaw muscles which would result.

In neither of the two cases alluded to has there been any

neuralgia in the branches of the ophthalmic division, although in both at the operation this part escaped division, and the same fact is to be noticed in the records of some of the cases given by Krause and others. In the following case, since the neuralgia had already involved the supra-orbital region, I took pains to remove the whole ganglion with the ophthalmic division, and unfortunately this was followed some time later by destructive ulceration of the cornea. The patient was a man, aged 64, who

FIG. 1.

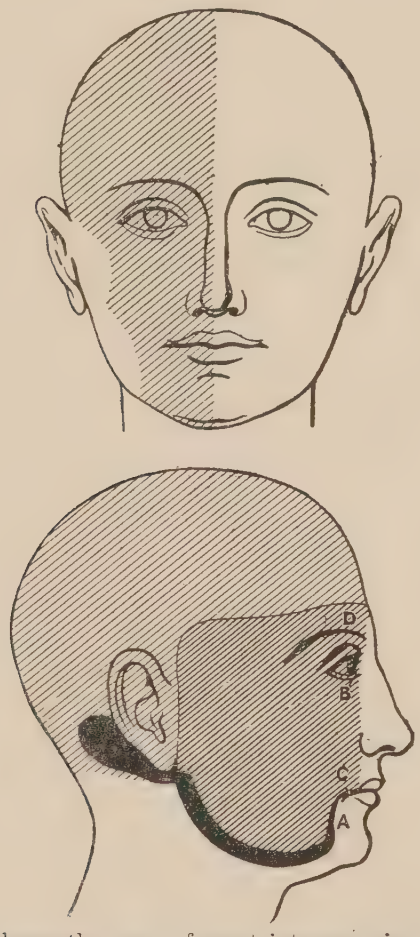


Fig. 1.—The lower diagram shows the area of most intense pain (in black, A), of severe pain (C, B, D), whilst the lightly-shaded scalp shows the part affected in the most severe attacks (an area only partly supplied by the 5th nerve). From the case described on p. 275. The upper diagram shows the anæsthetic area after removal of the Gasserian ganglion.

was sent to me by Dr. Simpson. He had suffered for five years from severe epileptiform neuralgia. The paroxysmal attacks started in the lower jaw, spreading to the whole side of the cheek, the lower part of the forehead, and temple, involved also the region behind the ear, and in a severe attack involved the whole of the right side of the scalp (*see* Fig. 1). This radiation of the pain to parts beyond the area supplied by the fifth nerve has

been noticed in many cases and does not contraindicate excision of the ganglion, by which operation it is put an end to. During the attacks there was usually marked lachrymation, and there had been more than once conjunctivitis on the affected side. The pain was not controlled even by opium, and the patient's condition was a very miserable one.

Excision of the Gasserian ganglion was performed in June, 1899, the chief difficulty met with being due to the extreme thinness of the dura mater. In raising this membrane from the middle fossa it was unavoidably torn, with the result that the cerebro-spinal fluid escaped. Owing probably to this fact the pressure of the broad retractor exerted a more deleterious effect on the brain than usual, and he had for some little time after the operation partial paralysis of the opposite arm and leg, which slowly improved. The wound healed perfectly and the neuralgia has been completely cured, the anæsthesia involves not only the right side of the tongue, palate, cheek, nose, &c., but also that half of the forehead, the cornea and conjunctiva. With regard to the eye, all went on well until several weeks after the patient had returned home. He then developed keratitis which slowly spread in spite of treatment, and ultimately, as the eye was useless, I excised it.

This case is the only one in which I have seen any ocular complication supervene, but the danger of it is well known, and is probably impossible to avoid when the cornea has been rendered completely anæsthetic. Even if the eye has been protected for a time by suturing the lids together the presence of dust containing perhaps septic micro-organisms on the cornea will not be felt and may set up ulceration.

Seeing that the epileptiform neuralgia involves mainly the superior and inferior maxillary divisions, it is a question whether it is not best to confine the operation to the lower part of the ganglion and not to make an attempt to wholly remove the upper part of the root and the proximal part of the ophthalmic division. This latter is much the most difficult stage of the operation owing to the cavernous sinus being readily opened, and several surgeons have recorded cases where one or other of the oculo-motor nerves were damaged in the attempt to thoroughly remove the upper part of the ganglion and the ophthalmic branch.

It is difficult to estimate the risk to life of the operation. One

writer, in 1898, collected 92 cases of operation on the ganglion by the temporal route, with 16 deaths (*i.e.*, a mortality of 15 per cent.), and 58 cases of operation through the pterygoid route (from below through the base of the skull), with 15 deaths, *i.e.*, nearly 30 per cent. This difference in mortality should alone lead us to discard the second method in favour of the first, but apart from this the temporal route is the only one by which it is really possible to expose and excise the ganglion. No doubt the latter is sometimes damaged and its branches severed by working from below after division of the zygoma, coronoid process, &c., but I am sure that in some cases operated on by the

FIG. 2.

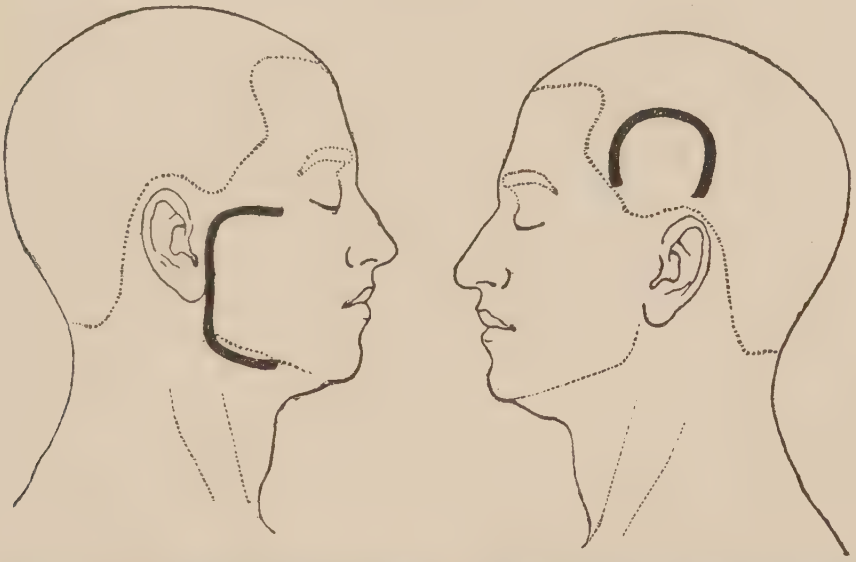


Fig. 2.—Diagram showing the two incisions used in approaching the Gasserian ganglion. The upper dotted line marks the edge of the hairy scalp. It will be seen that the incision for the pterygoid operation lies wholly on the side of the face (left hand figure), whilst that for the temporal operation is not only shorter, but will be concealed by the scalp. As stated in the text the pterygoid operation should be entirely superseded by the temporal one (Hartley-Krause).

most competent surgeons the ganglion has never been reached at all. Relief from pain may have been obtained for a few weeks, perhaps owing to the excessive blood letting, but the neuralgia returns without long delay as severely as before (*see* Fig. 2).

In the following case this unfortunate result had attended the operation, an extremely protracted one, carried out in two stages, and performed by one of the best operators in London. It had been believed that the ganglion had been dealt with, but the very limited anæsthesia following might well have been due to the division of nerves in the extensive skin incisions. The patient's neuralgia returned worse than ever. He took to injecting

morphia hypodermically in almost incredible amounts, and became physically and mentally a complete wreck. After his disappointing experience it was naturally difficult to induce him again to undergo surgical interference, and I expected that the operation would prove unusually difficult owing to the previous division of the bone round the foramen ovale, &c. This, however, did not prove to be the case. The middle fossa had been perforated about half an inch to the outer side of the foramen ovale, and the ganglion, with its branches, was quite intact. Nothing could be a stronger commentary on the difficulty of really dealing with the ganglion from below (by the pterygoid route). The only trouble I experienced in this operation was due to hæmorrhage, chiefly from the meningeal artery. The attempt to stop this bleeding by plugging the foramen spinosum with wax or a spicule of bone failed, and I therefore tied the external carotid in the neck. The two maxillary nerves were then divided and the ganglion excised. As an instance of the slight nature of the shock following this operation this patient was sitting up and writing a letter within 24 hours of it. Although he had got used to injecting eight or ten grains of morphia per diem he was fortunately resolute enough to entirely discontinue the practice as soon as he found the neuralgia had left him. He wrote six months after the operation :—"From being a hopeless invalid I have become as strong and well as at any time during my life, with practically no pain. I have never touched morphia since the operation and have not felt the least desire for it."

I have not myself had a death from the operation, but it must always be a tedious and prolonged one, and the patient is always in poor condition, worn out with pain and want of rest, depressed by the failure of a succession of previous operations to cure him, and probably the victim of morphia ; further, the examples of this major neuralgia are almost always past middle age (in two of my cases over 60). It is to be hoped that in the future such cases will not be subjected to a long series of palliative operations on the peripheral branches of the fifth nerve, or to such barbarous procedures as excision of the alveolar portions of the jaws, but that recourse will be had much earlier in suitable cases to excision of the Gasserian ganglion by the temporal route, the only method of treatment for inveterate neuralgia of the fifth nerve that offers a real prospect of complete cure.

I would venture to put forward the proposal that the so-called excision of Meckel's ganglion (always a very difficult and uncertain proceeding involving disfigurement of the face) should be given up in favour of intra-cranial division of the superior maxillary trunk just above the foramen rotundum. The relief to the neuralgia would more probably be permanent, and no disfigurement is left, since the scar is wholly hidden by the scalp. Of all my cases I can say that the only obvious sign of the operation having been performed is due to the atrophy of the temporal muscle, and that this is inconspicuous. The gap in the bone is soon diminished by new depcsit, and after six months the pulsations can hardly be detected through the skin; hence there seems no great object in performing an osteo-plastic resection.*

A fair-sized trephine is used, the pin of which is inserted in a line drawn from the centre of the external auditory meatus upwards and forwards over the root of the zygoma, so that the centre of the trephine is about an inch above the zygoma and $1\frac{1}{2}$ inches from the centre of the meatus.

With Hoffmann's cutting forceps the opening can then be readily enlarged, and the dura detached from the bony middle fossa until it and the brain can be sufficiently raised by means of a special broad retractor; the first landmark is the middle meningeal artery coming through the foramen spinosum, and placed 1 to $1\frac{1}{2}$ millimetres behind and to the outer side of the foramen ovale.

In order to thoroughly expose the ganglion it seems necessary to divide the meningeal trunk between two ligatures. This is not easy, owing to the depth of the artery (from 3 to $3\frac{1}{2}$ centimetres from the side of the skull), and the dura mater is apt to be wounded. Immediately in front of the foramen spinosum the inferior maxillary division will be exposed, and the landmark for this on the surface may be given as the front of the lower jaw condyle, or more accurately still, as the pre-glenoid tubercle. The dura mater is then further and carefully detached in a forward direction, so as to expose the superior maxillary division as it enters the foramen rotundum. The distance between the foramen ovale and the foramen rotundum averages in adult skulls $1\frac{1}{2}$ centimetres, whilst the depth of the latter from the side of

* Undoubtedly, if a large piece of bone is turned down after division by means of Gigli's saw, somewhat freer exposure is obtained.

the skull varies considerably, I have found it as little as 3 centimetres and as much as $4\frac{1}{2}$. Whilst working at such a depth in the narrow space between the dura and the bone, it is important to have checked all the hæmorrhage as far as possible, and the assistance of an electric lamp may be very useful. The operation is completed, after still further detaching the dura from the upper surface of the ganglion, by division of the inferior and superior maxillary trunks, then of the roots of the ganglion

FIG. 3.

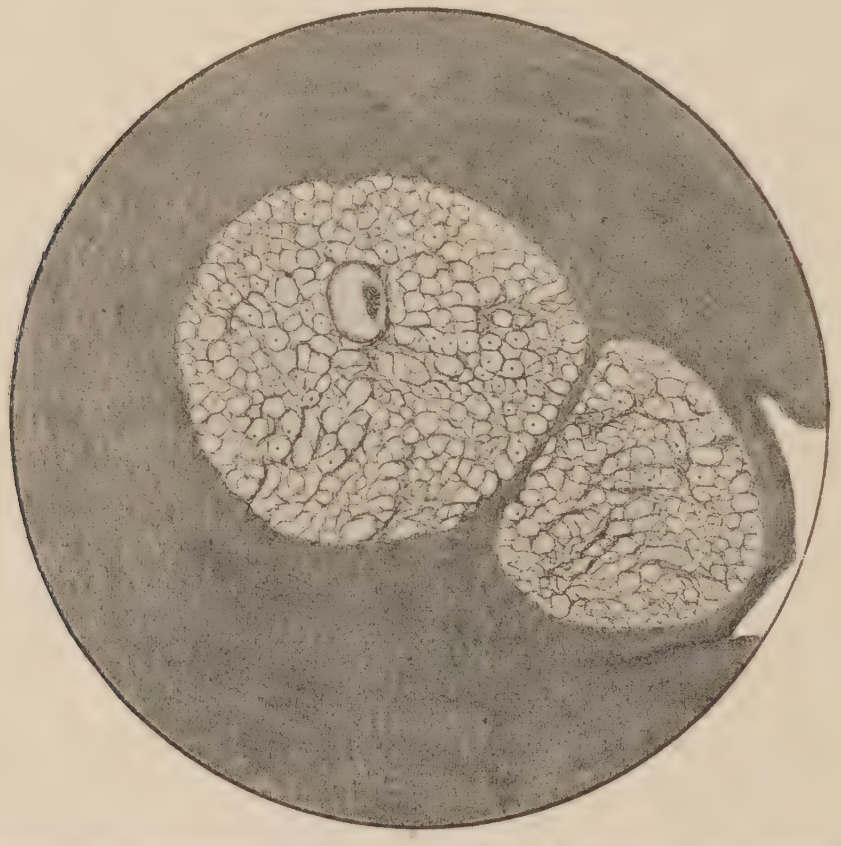


Fig. 3.—Section of nerve fibres from excised Gasserian ganglion. The tubules and small blood-vessels are seen to be normal, and though there is much fibrous tissue around the nerves this is quite normal. (From section prepared by Dr. Henry Head, F.R.S.)

as they enter its upper border, and finally by the removal of the ganglion itself. The ovoid flap of soft parts is then sewn in place, and, as already mentioned, the resulting scar lies, as a rule, within the margin of the hairy scalp. It may be noted that the portion of bone removed extends forwards to exactly an inch from the exterior angular process of the frontal bone and downwards to the zygoma, whilst the chief landmark for the ganglion is that part of the zygoma immediately in front of the condyle of the lower jaw.

Undoubtedly the chief difficulty of the operation is due to

hæmorrhage, the blood welling up from the dura mater in a most troublesome manner. Professor Krause advises that the patient should be kept in a sitting posture (for instance, in a dentist's chair) during the whole operation. I have tried this in one case with what seemed to be good effect as regards diminution of oozing, and with no extra trouble as regards the anæsthetic. I see no reason against preliminary ligature of the external carotid artery.

Dr. Henry Head, F.R.S., who has greatly assisted me in many ways with the cases narrated, has kindly examined the ganglia

FIG. 4.

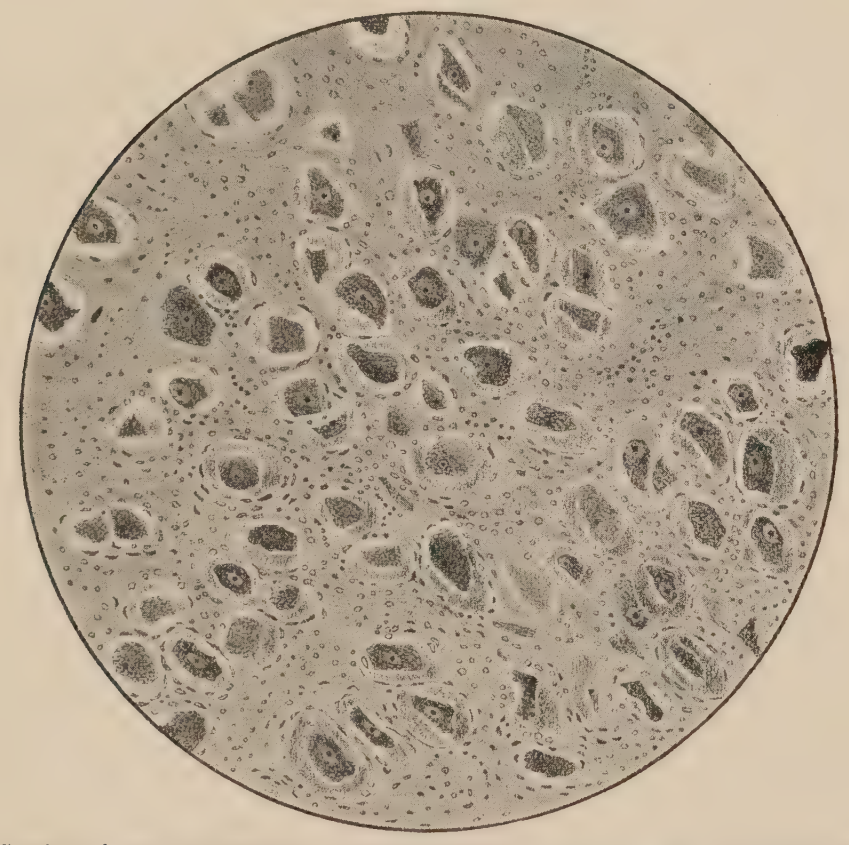


Fig. 4.—Section of ganglion-cells from excised Gasserian ganglion. The cells and intervening connective tissue are quite normal. (From case of severe epileptiform neuralgia.)

removed. I do not think it is possible to say that in any of the sections definite pathological changes can be made out. In none of them was arterio-sclerosis proved, the ganglion cells seemed perfectly normal, and whether the fibrous tissue surrounding them and the nerve bundles was in excess or not is, of course, a very difficult question to decide. Certainly in not one of the specimens was there marked evidence of inflammatory or degenerative changes (*see* Figs. 3 and 4).

In conclusion, one would emphasise the fact that the results of excision of the Gasserian ganglion are undoubtedly more permanent as regard the relief of the neuralgia than any other operations practised on the branches of the fifth nerve outside the cranium. Of this there is now abundant evidence.

It is not too much to say that whilst excision of the ganglion by the temporal route is one of the most difficult of surgical operations, it is also one of the most valuable.

The PRESIDENT said he was convinced that the term "neuralgia" was applied to many cases which were really organic. Although nothing very definite could be recognised in the sections which the author of the paper had shown them, yet he believed that there must have been some structural change to account for the pain. The difficulty of treatment in these distressing cases was known to them all, and he expressed satisfaction that operative procedures could, at the present day, be successfully practised for the relief of, at any rate, a certain proportion of them.

Dr. A. E. SANSOM asked in what class of cases the author thought this operation would be justifiable. He was obliged to use the term "neuralgia" for want of a better, because at present there were no means of differentiating the organic and the so-called functional cases.

Mr. HUTCHINSON, jun., in reply, agreed that it was unsatisfactory not to be able to ascertain the true pathology of these cases, but, on the other hand, he did not think it was fair to strain a point and assume that the very minute changes sometimes seen in the sections had any causal relation with the pain. He referred to a case recorded by Mr. R. W. Smith in his famous monograph, in which, in a patient suffering from this form of neuralgia, a neuroma was found, after death, which involved the fifth nerve and the Gasserian ganglion. Such a case pointed to there being a physical basis for severe and persistent neuralgia, though it could not, as a rule, be detected. As to the cases in which such a severe operation was justifiable, he said that none of his own cases had lasted less than five years, but he would not advocate waiting so long as that. All five patients were reduced to such a low ebb that they were running a needless risk; moreover, there was the danger of their becoming addicted to morphia; indeed, the operation was the only alternative. Of course, some cases of circumscribed neuralgia were to be treated by less heroic operations. He had operated upon two cases in which trephining the lower jaw and removing the inferior dental nerve entirely cured the neuralgia, which was confined to that particular branch. Sometimes it was the upper jaw that was the seat of the pain, and in such cases one resected the infra-orbital nerve; but where the pain affected the two lower divisions, with radiations along the ophthalmic divisions, and where it had an epileptic character and had lasted at least a year, causing much suffering and deterioration of health, he thought the operation should be done without further delay. The operation caused very little deformity, the scar being hidden by the hair. The surgery of the fifth nerve, he thought, would be simplified by their discarding a number of very ingenious but very complicated operations in favour of this much simpler procedure.

A CLINICAL STUDY OF THE CAUSES OF THE FIRST SOUND OF THE HEART.

By Alexander Morison, M.D., F.R.C.P. Edin.

In the beginning of 1896, when in charge of my colleague Dr. C. E. Beevor's beds in the Great Northern Central Hospital, the following case was admitted under my care, and to some points in it I wish to refer in connection with the theme of this paper.

A housemaid, aged 22 years, was admitted into the hospital on January 6th, 1896. She was stated never to have had any serious illness until about two months previously, when she manifested shortness of breath, lassitude, and loss of appetite, together with a progressive anæmia. About three weeks later the acute phase of her illness began, when she complained of pains in the abdomen, vomited daily, had headache, was feverish, and at night was somewhat delirious. She steadily became worse, refusing food and losing weight until the date of her admission. On admission she was found to have tenderness in the left hypochondriac region. The area of splenic dulness was increased downwards and forwards two-thirds of the way to the umbilicus, but could not be palpated on account of reflex rigidity of the abdominal muscles induced by the excessive tenderness to touch. The other abdominal organs presented nothing notably abnormal except the kidneys, the urine showing a trace of albumen and having a specific gravity of 1018. The examination of the lungs gave a negative result, but respiration was very rapid and shallow, being at the rate of 84 to the minute. The heart's action was also very rapid but regular, and its impulse was recorded as heaving and in the nipple line. The heart-sounds were also described as having been "loud," and without bruit. The pulse was soft, regular, and had a rate of 150 to the minute. There was some œdema of both feet. The patient was conscious, had equal pupils which reacted to light, exaggerated knee-jerks, continuous ankle tremor of a neurasthenic type, and well-marked superficial reflexes generally. The temperature was 101·8° F., and there was no cutaneous eruption.

It is not necessary in the present connection to enter with

much detail into the subsequent clinical history of the case other than that affecting the heart-sounds, beyond stating that the temperature ranged between 97° and 103° until the two days preceding death, when it fell as low as 95°, rising on the day of death (January 16th) to 98·5°; that the area of hepatic dulness increased; and that a loud superficial friction, manifest on auscultation, appeared over the gastro-hepatic area which was shown at the necropsy to be transmitted from the perisplenic region through the intervening organs which acted as conductors of sound.

As regards the cardio-vascular signs, the following facts were noted:—On January 11th the first note is made of the condition of the heart after the record made on admission, when the sounds are described as being "very loud and hollow," and associated with a "slight systolic bruit at the apex." On the 12th it is written that there was a "very high-pitched systolic bruit at the apex quite different in quality from that observed the day before," and that "the pulse and respiration continued very rapid." On January 15th the pulse, it is stated, could only just be felt at the wrist, and the heart-sounds are described as good, and the action as regular and at a rate of 108. The respiratory rate was 60, and the abdomen moved with the descent of the diaphragm. The morning temperature was normal and fell to 95·4° on the same evening. The patient was then dying from exhaustion, and on the next day died. The necropsy was made by the pathologist to the hospital (Dr. Freyberger) on January 17th, 20 hours after death. All the organs of the body were healthy with the exception of the spleen, the heart, and the kidneys, and the description of these I copy verbatim from the register:—

Spleen.—Double size, firmly adherent to peritoneum by means of perisplenitic pseudo-membrane; a large anæmic infarct running like a band across the whole of the sub-surface. A smaller cone-shaped one near the apex. No suppuration. Consistence somewhat increased.

Kidneys.—Both cloudy. Capsule not stripping readily. Cortex diminished. Glandular structure indistinct. A small anæmic infarct. No metastatic abscess.

Heart.—In the pericardial cavity about 6 ounces of clear serum. A somewhat recent pericardial adhesion posteriorly about half way between the coronary sulcus and the apex; a much older one (which has evidently become detached) at a similar place anteriorly. There are milky spots anteriorly over the right ventricle. Right ventricle.—On section muscle about a quarter of an inch thick, healthy looking, consistence not diminished. Pulmonary valves competent, natural. Tricuspid natural; admits three fingers. Loose clot and much fluid blood in right ventricle.

Right auricle natural. Left ventricle somewhat contracted. On section muscle half an inch thick, of a greyish-red colour, consistence not diminished. On the left lateral semilunar valve a large lump of coagulated fibrin of a verrucose appearance, taking up the entire ventricular surface and dragging the valve downwards. The fibrinous deposit is continued on to the right lateral aortic valve, which with its anterior half participates in the formation of this excrescence which sends a long, free, tongue-like process down into the ventricular cavity. A similar verrucose excrescence, the size of a pea, on the posterior valve, occupying the site of the nodulus Arantii. Mitral valves.—The mitral cusps are both involved in the process, the valves shortened, and their free edge covered with more cauliflower-like excrescences, which are especially extensive on the auricular surface of the aortic cusp and the right half of [the] posterior cusp. The valves are nowhere perforated, the chordæ tendineæ of [the] aortic cusp shortened, and on some of them there is also precipitation of fibrin in consequence of peri-tendinitic roughness. The posterior auricular surface roughened and puckered, with some lamellar fibrinous precipitate in places.

This case was evidently one of so-called malignant endocarditis, ingrafted in an anæmic subject upon old and extensive lesions of the aortic and mitral valves, probably of rheumatic origin, although no history to this effect could be elicited. The clinical observations were made by myself and by Mr. Randall Wadd, who had been house physician to the Brompton Hospital for Consumption before coming to the Great Northern Central Hospital. I mention these particulars to increase the value of the observations which refer to the general clearness and audibility of the heart-sounds. This condition attracted my attention the more, because I had at that time recently assisted the late Sir Richard Quain in the preparation of a paper, which he read before the Royal Society on June 3rd, 1897, on the mechanism by which the first sound of the heart is produced. In the case I have recorded, except upon the rare occasions on which a systolic bruit was heard at the apex, both sounds were, as has been stated, clear and even, as the notes tell "loud" and without bruit. After the evidence of destruction in the aortic and mitral valves revealed at the necropsy it became a matter of interest to discover where and how these apparently normal sounds originated. That the clear second sound could only arise at the pulmonary arterial orifice was manifest, and its clearness or loudness were probably in a measure due to abnormal tension in the pulmonary circuit and the absence of disturbing murmur in the left heart, together with its superficial position. But the explanation of the clear *first* sound was as much a puzzle to me as a clinician as the

satisfactory explanation of the first sound of the heart still is to many experimenting physiologists. I therefore thought some light might be thrown upon the cause of this sound by studying it from the clinical point of view, and have examined a number of cases with this object. A large proportion of these cases was taken from my clinic at the Paddington Green Children's Hospital as a matter of personal convenience rather than of choice, but the circumstance is not to be regretted because of the variety such subjects present and the ease with which sounds are conducted through their chests. The younger the child, indeed, the more easily are both heart-sounds radiated all round the chest, but there are, of course, points at which one or other sound has its position of maximum audibility. In the case of the first sound, as we know, this is at the apex of the heart, and in the case of the second at the base. The general principle that a greater noise obscures a less is true in the case of the heart as in that of all other substances or media which are resonant. The possible media for the generation and conduction of sound in the case of the heart are its fibrous structure and valves, its muscle, the blood circulating through the organ, and the thoracic case which contains the whole with its solid and gaseous contents and surroundings. We may shortly consider these possible factors *seriatim*.

1. As regards the valves. In the case of mitral systolic bruit both sounds may be universally audible in front, even including the apex area, when the bruit is not loud, and in this case to the right of the sternum both sounds may be heard without associated bruit. This may be so even when regurgitation through the valve is sufficiently marked to be audible in the left paravertebral groove. But in such case the second sound is not audible in the back. When the second sound is audible in the back in mitral regurgitation, it is usually at the level of the scapular spine and denotes accentuation of that sound and all that that implies. When the second sound is thus accentuated the first sound is usually obscured, and systolic bruit and the evidences of dilated hypertrophy are generally well marked. When the mitral bruit is presystolic in time and even associated with presystolic thrill, both sounds may be universally audible in front unless the noise of the bruit be considerable, in which case the first sound is masked and the second audible as a simple or reduplicated sound.

In the case of a double mitral bruit both sounds may be audible outside the apex area, while the second may be heard at the apex and in both paravertebral grooves. With aortic systolic bruit the first sound may be obliterated in the aortic area and the second heard, while both may be audible in the pulmonary area and also in front, provided the systolic bruit be not very loud. Both sounds may also be distinctly audible all over the præcordia when the aortic bruit is diastolic, unless the diastolic bruit be loud, when the second sound is as universally masked. If with a loud diastolic bruit the heart suddenly fails, as it does in anginous attacks, the pulmonary second sound becomes audible and even accentuated, and is again masked when the bruit returns with the recovery of the heart. In double aortic bruit there is most abolition of the heart-sounds because the noise is often considerable. With fair ventricular systolic force, however, in these cases, the closure of the pulmonary valve is usually marked by a detectable second sound. When an aortic systolic bruit is associated with a mitral systolic bruit the second sound may be clearly audible, both noises being systolic. The same is true of systolic pulmonary murmur with tricuspid regurgitation, of which I have observed one case verified by *post-mortem* examination. In this instance, however, the aortic second sound could only occasionally be detected with difficulty on account of a diastolic pulmonary bruit being also present. Hæmic systolic bruits at the base of the heart may conceal the first sound to one or both sides of the sternum, but in such case both sounds are easily audible at other points of the præcordia. In cases of congenital malformation of the heart when bruit is present it is usually (always so far as my experience goes) systolic, and if loud may mask the first sound, but not by any means invariably. Even when the systolic bruit is loud the first sound may at times be easily heard through it. The second sound is usually very distinctly audible, because accentuated, and both may be heard over a wide area. Thus in the case of a patient under my care in the Children's Hospital there was a loud systolic bruit best heard over the cartilage of the fourth left rib, but both sounds could be heard universally over both the front and back of the chest with remarkable clearness, although the first sound was masked but not lost in the area of loudest bruit. On the other hand, considerable congenital defects may be present, as shown by subsequent necropsy, and even in association with well-

marked cyanosis, without presenting any bruit. In such cases both sounds may be widely heard, and only differ from the normal in exhibiting an accentuation proportionate to the cardiac hypertrophy and vascular impediment present.

2. As regards the influence of the cardiac muscle in the production of sound. The force of systole in modifying the pitch or audibility of bruit is a well-known fact. To detect an indistinctly marked bruit we often cause the patient to walk about or exert himself in some manner to increase the force of systole and with it the audibility of murmur. It may frequently be observed also that as the force of systole wanes bruits usually easily detected and of an average and steady pitch become less audible or even lost. In the stage of exhaustion in protracted fevers the audibility of the first sound is frequently much impaired. This is also the case in feeble persons in advanced life and during failure of the circulation from anæsthetics or other cause. In all these cases the second sound is usually more audible than the first if the arterial valves be entire.

3. The clinically observable influence of the blood in the production of sound and in the obscuration of sounds is perhaps the most easily determinable of all the factors mentioned so far. The weakening of the heart-sounds from loss of blood in surgical and obstetric practice is too well known a phenomenon to call for further comment, while the possible loudness of bruits or noises at defective orifices has been sufficiently emphasised in a previous section. In the case with which I opened this paper, indeed, the peculiar audibility of the heart-sounds was no doubt chiefly owing to the abolition of the bruits in the left ventricle by the failure of impulse in the circulation, although this does not explain fully either where or how the persistent sounds were originated.

4. The fourth and last factor which I have to mention in connection with a clinical study of the cause of the first sound of the heart is the possible influence of the thoracic case in which the sound-producing organ is contained. All clinical conditions may be regarded as ready-made natural experiments for the study of the physician, but on this point nature at times makes an experiment which is as artificial as any performed in the physiological laboratory. Personally, I have never had an opportunity of observing a case of ectocardia, but such have fortunately been

carefully studied. Mr. C. G. Grant observed a case in 1896, an account of which with an accompanying photographic illustration may be seen in the 'British Medical Journal' of December 5th of that year (p. 1639). In this case, which was that of a fœtus born alive about the eighth month and which lived for six hours, the heart, which was "external to the chest wall" and devoid of pericardium, pulsated vigorously. The sounds at the base were clear and distinct, no murmur being audible; auscultation at the apex was, it is stated, "impracticable owing to the exaggerated movements." The influence of the chest wall in contributing towards the sounds of the heart as determined at the bedside may therefore be set aside as a negligible or inoperative quantity. Where, then, and how did the clear heart-sounds in my case originate? In the first place, where?

As has been already stated, there can be little doubt that the second of the two normal sounds detected originated at the pulmonary arterial orifice, for the aortic valves were too much diseased for competent closure. The situation of the first sound heard remains to be determined. As has been mentioned the sounds were recorded at times as peculiarly "loud." Now, it is a characteristic of noises at the pulmonary arterial orifice that they are very easily heard on account of that vessel being comparatively near the surface. With the accentuated *clacquement* of the second sound at that orifice we are all familiar. It is loud to the ear and palpable by the hand. It may, indeed, be used when thus marked for determining the position of the base of the heart without other aid. Bruits at the same orifice though rarely observed are loud and easily perceptible, and may be associated, as in a case which I published in the 'Transactions of the Pathological Society' (1876), with a very coarse thrill. Similarly other sounds originating in the right ventricle if freed from obscuration by competitive noises might be expected likewise to be distinctly audible. In my case this freedom from competitive noise might be supposed to be secured by the feebleness of ventricular action in the diseased left ventricle which, together with the reduced aortic pressure from valvular reflux, prevented the generation of bruit at the defective orifices. It may be asked, then, if the left ventricle was so greatly disabled whether the right did not also share in its enfeeblement; and this must undoubtedly have been the case. If, therefore, the first sound originated in the right ventricle it must

at any rate mainly have been due to some circumstance or circumstances other than contraction of the cardiac musculature of that chamber. What was this circumstance, or, it may be, what were these circumstances? This leads to the consideration of how the first sound of the heart originated if it be assumed or granted that it arose chiefly in the right ventricle, as seems probable.

In making a few remarks upon this point I desire to avoid all appearance of dogmatism, and merely wish to mention some circumstances as premisses which appear to lead to a rational conclusion. The argument will be from the known to the unknown, or, at all events, the disputed. My case, merely taken as an example, shows that either series of semilunar valves may alone be the site of the second sound of the heart, because the aortic valves were quite incompetent in that case and therefore silent. When the second sound is accentuated, moreover, the sound may be felt to be coincident with closure of the semilunar cusps if the hand be placed on the chest, while the ear listens through the stethoscope. The second sound, therefore, is due chiefly to an incident in diastole, not to diastole as a whole. It appears possible, therefore, that some incident in systole and its surroundings may play a greater part than others in the production of the first sound of the heart. The possible factors have already been stated to be:—(1) The valvular and fibrous structures of the heart; (2) its muscular action; (3) the blood waves; and (4) the thorax.

As regards the first point, we have seen that the first sound may be audible in all forms of valvular disease at one or other part of the chest, unless obscured by bruit, which is a very different matter from being abolished or replaced by such. Even hæmic bruit without any valvular incompetence may obscure the first sound of the heart. There is, however, a fallacy involved in drawing a positive conclusion from this consideration, because to exclude the participation of the valvular structures from taking part in the production of the first sound there should be evidence of all four being greatly incompetent at the same time. Such a condition, if present in considerable degree, would be incompatible with any but the shortest life, and I am not aware that it has been observed by the clinician, or even by the experimental physiologist, although I have myself known the aortic, mitral, and tricuspid valves to be simultaneously much disorganised. But

even if all the valves were diseased at one time the character as well as the degree of the disease would have to be taken into consideration in deciding the point, for the stiffened flaps of an indurated valve may be an excellent vibrating structure, or serve in a great measure to divide the blood-stream as under normal circumstances. It is certain that the first sound may be audible when representative valves (auriculo-ventricular and semilunar) are diseased, but some share in the generation of the first sound of the heart on the part of the valvular structures cannot be excluded as a result of clinical observation, although that share has probably been over-estimated.

As regards the muscular factor my case proves that the first sound of the heart may be clearly audible when the muscular force of the ventricles is at zero. Congenital cases, moreover, show that gaps in the cardiac septa do not interfere with the generation of a normal first sound, although resulting bruit may at some points obscure it. Sound emitted by the cardiac muscle seems therefore, on clinical evidence, not to be an important factor in the production of the first sound of the heart as conveyed to the ear by the stethoscope.

The influence of blood waves on the other hand in producing sound is clearly shown by the character and variation of the bruits or noises which arise when the blood current is transmitted through diseased or malformed apertures. As, moreover, the distinction between a noise and a tone is merely one of vibration and the fluid medium—the blood—is that to which such vibration must in greatest measure be communicated, the important part played by the blood itself in the production of the first sound of the heart appears on clinical grounds to be unquestionable.

The last point which has to be weighed—namely, the influence of the thorax, the air-containing cavity which holds the heart in producing the first sound, is easily disposed of. Mr. Grant's case already quoted proves not only that the chest wall has little or nothing to do with the production of the sounds of the heart, including the first sound, but also reveals another very interesting fact—namely, that both sounds may be clearly audible in cases of ectocardia at the base of the heart, when the apex from its mobility is incapable of being satisfactorily auscultated. Were it possible to place the whole mechanism in vacuo without the heart ceasing to pulsate, no doubt all sound to the ear would be abolished very

much as the movements of a watch would cease to be audible under like circumstances. But the general question of the conductive power of air may be dismissed as foreign to the present inquiry. In Brakyn's* apparatus air took the place of blood and emitted the systolic and diastolic sounds. In nature blood takes the place of air.

The conclusion, therefore, which appears to flow from this clinical study of the question seems to be that the impulse given to the blood by the muscular action of the heart gives rise in it, and in the vibratory structures containing it, to vibrations of a certain quality which result in that tone which is synchronous with cardiac systole, and usually called the first sound of the heart, and that the largest share in this result must be attributed to vibrations in the blood itself, to produce which at a normal rate valvular support of the vibrating blood-columns is necessary. The study of some congenital cardiac malformations especially appears to support this conclusion, because with considerable parietal defects and bruits caused by such the sounds of the heart may be well heard so long as the division of the blood is fairly maintained by the valves. This conclusion from clinical observations and argument is essentially the same as that reached by the late Dr. Arthur Leared, formerly one of the physicians to the Great Northern Hospital. I cannot, however, agree with him when he writes that the events which occur in the ventricles and at the arterial orifices have no more to do with the generation of the sounds than "the vibration of a door [has] with the sound produced by air [passing] through its keyhole."† There is a difference in the noise made by a door slammed to and one pushed open. The former represents the second, the latter the first sound of the heart, and in both the character and distribution of the vibrations differ.

The conclusion I have drawn differs somewhat likewise from that arrived at by my late friend Sir Richard Quain in that I cannot attribute so much importance as he did to the impact of the ventricular blood against the closed semilunar valves only and the superincumbent blood-columns, although I think there can be little doubt that it is at this point that the most important

* 'The Lancet,' November, 1849.

† 'Essay on the Sounds Caused by the Circulation of the Blood,' London, 1861.

portion of the vibrations arise which result in the production of the first sound of the heart. Dr. Leared appears to me to have attributed too much and Sir Richard Quain too little importance to the sound-producing qualities of the blood itself. The truth appears to lie between the views of these two observers, and may include as a very subsidiary factor a note derived from the muscular contraction of the organ. The estimation of the last point, however, by the clinician is a very difficult matter.

I have purposely refrained from reference to the results of experimental physiology on this occasion, not because I underrate its great importance, but because it has been my desire to show that in the advancement of scientific medicine the careful study of the ready-made experiments which come under the cognisance of the clinician (a field open to all departments in the profession) may be fruitful of valuable results. The power to disintegrate factors, however, which belongs to the experimenting physiologist renders his assistance necessary to a precise knowledge of the causes of vital phenomena. Some of these, and notably Halford in his valuable essay on 'The Action and Sounds of the Heart' (1860), are largely in agremeent with the conclusions set forth in this paper. They dissent from them chiefly in the greater importance which they attach to vibrations of the auriculo-ventricular valves and of the blood in the ventricular cavities.

Dr. A. E. Sansom said it was remarkable that a case of ulcerative endocarditis could run its course without any manifestations of murmurs. That there might be great destruction of the valves, and yet no alterations of the normal sounds or the production of murmurs, was a very important clinical fact. Fortunately, there were other characters which enabled them to arrive at a diagnosis even in the absence of murmurs. He agreed with the author in thinking that the sudden first sound might have been produced in the right ventricle, and that the tricuspid valve was not at all altered. In this case they might exclude from the production of the sound any undue tension of the mitral valve, as well as tension in the aorta, but tension might easily be produced in the right side, and the thin membranons tricuspid was a very likely source of the sudden tension sound that was heard. A tension sound was still possible both in the auricle and also in the arterial circulation. He asked whether it was not a plausible hypothesis that, in the production of the first sound, tension was produced somewhere—usually in the two ventricles contracting simultaneously—and, if the mitral valve were destroyed, there were other structures which could be thrown into tension, especially the tricuspid valve, because that was the thinnest membrane. For the production of a loud first sound there must be sufficient ventricular muscle, but not hypertrophy, because a hypertrophied muscle did not give a strong sound. That there was such a

thing as a muscle sound he would agree, but it was not a loud sound. The sound of tension was very much obscured by a thick-walled ventricle, whereas in a thin-walled it was increased. The question of adequacy came in. A comparatively weak ventricle, whether right or left, would produce a short and sudden sound, but a very strong one did not produce that sudden sound, because the thickness of muscle would impede the conduction to the ear. He could not bring himself to believe that collision of the blood had anything at all to do with it except as an intermediary. He held, then, that the first sound was one of tension, and that when the sound was dull and prolonged it was because it was masked by a great thickness of muscle, and the very slight sound of the contraction of muscle was superadded.

Dr. Robert Maguire was glad that Dr. Morison had referred to the paper by the late Sir Richard Quain, though he had not quoted it quite correctly. According to Quain, the first sound of the heart was caused by the charge of blood, contained in the ventricle, being driven like a rifle shot through the column of blood in the aorta or pulmonary artery, and had nothing to do with either the valves or the muscular tissue. There was no muscle which could make a sound at all like that which the ventricle was supposed to produce. He pointed out that when the heart was removed from the body and only propelled air, the sound was by no means the same. If they accepted Quain's view, it followed that the sound was produced by the impact of one column of blood against the other, and not by its impact against the containing structures. If this view were accepted the relationship of the first sound to murmurs was much easier of comprehension.

Dr. Morison, in reply, said that he had endeavoured to point out that the sounds were probably produced in the right ventricle. The question was necessarily very difficult to settle clinically. The evidence was perhaps in favour of the arterial valves being nearer the seat of production, and he agreed that tension had probably something to do with it, though that point was perhaps unduly emphasised by Quain. He presumed that tension communicated vibration through a medium, and that medium appeared to him to be the blood. In experiments the heart sounds were very perfectly imitated by air, but in the absence of blood and air he questioned whether tension would produce any sound at all. The only difference between the views of Sir Richard Quain and Dr. Leared was that the former attributed more importance to the vibrations communicated by the tension of the valve stroke than the latter did. He thought that the area of audibility of the pulmonary second sound was more extensive than Dr. Sansom had suggested, for the simple reason that one could hear a reduplicated sound over a wide area. He was convinced that Quain was, in the main, right, and that in the course of a few years the text books would adopt his views in preference to those of others whose conclusions were based upon work in physiological laboratories.

May 21*st*, 1900.

THE ANNUAL ORATION—"SCIENCE AND MEDICINE IN THE EIGHTEENTH CENTURY."

By James Kingston Fowler, M.A., M.D., F.R.C.P.

Mr. President and Fellows,—A request to undertake a duty in connection with this ancient Society is an honour which none of its Fellows can refuse, but to prepare an Oration worthy of remembrance with many of those which have been delivered on this anniversary with scarcely a break for 126 years, is a task of difficulty, and one to the discharge of which I can bring no adequate ability; but for the opportunity I tender the Society my grateful thanks.

In the choice of his subject full freedom is given to the Orator, and standing as we do upon the threshold of a new century, it would appear natural to review the advances which our art has made in that which has so nearly come to its end, but to do so would be to travel a familiar and well-trodden road, and I prefer rather to endeavour to present a picture of the state of science and medicine in the eighteenth century, a time with which we are all probably less familiar, feeling assured that you will contrast it with that which obtains to-day.

The century which ended on the 31st December, 1800, was a period of remarkable progress for the British nation, but of this little was noticeable in its earlier years.

Abroad, the military genius of Marlborough gave lustre to our arms; at home, in 1715 and 1745, the hopes of a restoration of the fallen Stuart dynasty were fortunately crushed, only to be revived in the breasts of a few amiable and harmless enthusiasts, who a hundred and fifty years later, upon a certain anniversary, are found hanging wreaths and garlands around an equestrian statue in the neighbourhood of Trafalgar Square. It witnessed the origin of the art and mystery of stock-jobbing, "the whole city was turned into a corporation of usury," and there arose a bubble which, like its prototype blown from a soapy sea, grew, before it burst, to an enormous size, and was followed by others

floating in the air. "Exchange Alley was filled with a strange concourse of clergymen, churchmen and dissenters, whigs and tories, physicians and lawyers, tradesmen, and even females; all other professions and employments were utterly neglected." Other arts and mysteries have been lost in the succeeding ages, but this is a precious possession which remains with us still as a monument to the unchangeability of human nature.

The fabric of the State now began to be more firmly "based upon the people's will," but it has been reserved to our present beloved Sovereign to show to the nation and to the world the real nature of a constitutional monarchy, and that under its beneficent rule Liberty, Equality, and Fraternity are not synonyms for civil strife, plunder, and bloodshed.

In the middle of the eighteenth century, at the Siege of Arcot, the first was heard of Robert Clive, who as an idle boy had been shipped off by his parents to India, as a writer in the Company's service, but who was destined to change that same Company of English traders into the Sovereigns of Bengal, and thus to lay the foundation of our Empire in India—the India which, in spite of the "perish"-ers, we still hold, and whose native chieftains to-day vie with each other in their devotion to the British Crown and in their desire to be allowed to serve the Empire in times of danger. The century, too, saw the dream of French dominion—not only in India, but also in America—disappear, as at Quebec the great spirit of Wolfe passed away in the moment of victory on the heights of Abraham. It also, alas! saw those same American colonies, but not Canada, lost to the mother country, through the fatuous folly of a king, of whom it has been said:—"In ten years he reduced government to a shadow and turned the loyalty of his subjects into disaffection. In twenty he had forced the colonies of America into revolt and independence, and brought England to the brink of ruin." A king whose intellect was such that he once asked, "Was there ever such stuff as Shakespeare?" and whose generosity is indicated by the fact that he regarded as a personal affront a proposal to raise a public monument to one of England's greatest statesmen.

Then, as now, the world watched with scarcely disguised pleasure the misfortunes of England, but in the eighteenth century, as in the nineteenth, saw to its astonishment the spirit of the nation rise from defeat and disaster stronger than ever

before, determined to prove that if not a military nation we are yet a martial race, and ready, now that America was lost, to seek elsewhere—in Africa, New Zealand, and Australia—fresh fields for her enterprise.

During the closing years, and more especially in the final decade of the century, the results of the wide diffusion of intelligence for which it was so remarkable were seen in a broader conception of national life and of "the rights of man," and in the development of a sense of human brotherhood and a hatred of oppression, and of the power of a privileged class, which led to the French Revolution with all its attendant horrors. The excesses by which the movement was attended gradually alienated the sympathy with which it was at first regarded in this country, and when in 1793 France declared war against England, it found the country united and almost the whole of Europe in alliance with it. But four years later the fortune of war was against the allied forces, and on land the arms of France were everywhere victorious. In 1797 "England was left without a single ally, her credit had sunk to the lowest ebb, and the alarm of a French invasion brought about a suspension of cash payments on the part of the Bank."

There were pessimists and croakers in those days as in our own, but there were no evening papers. Again the nation proved equal to the emergency. Two millions were raised by public subscription for the expenses of the war, and both on land and sea victory attended our arms. The Spaniards were defeated in 1797 by Jervis off Cape St. Vincent, and in 1798 Admiral Duncan almost annihilated the Dutch fleet off Camperdown. The valour of the enemy in that battle was worthy of their ancient renown, and at the close of the succeeding century their descendants have shown that they still possess the fighting qualities of their ancestors. England then stood alone in the war, but the victory of Nelson at the Nile brought allies to her side, and for a time, by the union of the Russians and Austrians, the French were driven back across the Alps and the Rhine, and Italy was lost to them.

The English expedition against the Dutch was, however, a complete failure, but the splendid defence of Acre by Sir Sydney Smith led Buonaparte to abandon his army in Syria and return to France and Paris, where, having overthrown the Directory, he

was appointed first Consul of the Republic. His offer of peace having been refused by this country, in 1800 he crossed the St. Bernard, defeated the Austrians at the Battle of Marengo, and forced them to conclude a peace which fixed the frontiers of France at the Rhine. Such was the political condition of Europe in the year 1800.

Science.

The pursuit of war abroad and the presence of civil strife at home do not appear to have materially affected the progress of science; news travelled but slowly, and men were not then connected with their fellows upon the field of battle so nearly as to enable them fully to realise and almost to see the horrors of war.

The two great sciences of chemistry and electricity gradually emerged from the tentative stage in which they had been left by the ancient writers. Gilbert, the inventor of the term "electricity," discovered the attraction and repulsion which certain bodies, such as amber, exert on other substances when rubbed; Grey showed that some bodies have, and others have not, the power of conducting electricity, and he also helped to differentiate the positive from the negative pole. Benjamin Franklin, at a later period, by his famous experiment with a kite, established the identity of thunder and lightning with the phenomena of electrical discharge, whilst his conception of electricity as a single fluid helped to explain more clearly the nature of positive and negative polarity.

To this period also belong the invention of the Leyden jar (1745), the discovery of the phenomena of electrical induction, and of the fact that the shock of the torpedo fish is due to an electric discharge—an observation which led to the construction in 1776, by Cavendish, of an artificial torpedo by which the actions of the living animal were imitated.

Chemistry.

Chemistry in the early part of the eighteenth century consisted of a number of separate observations not yet formulated into an independent science. Boyle, in his work published in 1661, entitled 'The Sceptical Chemist,' had done much to disprove the

false doctrines which had hitherto prevailed, and Stephen Hales had clearer ideas than those who had preceded him of the constitution of atmospheric air; but it was not until the discovery by Joseph Black, in 1754, of "fixed air," or "carbonic acid" as it was subsequently termed, and 20 years later by Priestley of oxygen, or "dephlogisticated air," that the composition of the atmosphere was determined, and at the same time the science of chemistry was reconstructed.

The history of the term "phlogiston" should have some interest for us as medical men, as it survives in the word "antiphlogistic." It was introduced about the end of the seventeenth and early part of the following century by two foreign chemists, Becher and Stahl, to explain the phenomenon of combustion, phlogiston being the substance which was assumed to be lost by bodies undergoing destruction by fire. The theory, though erroneous, served for a long time as a working hypothesis, and was not finally disproved until the end of the century. Some time after his discovery of oxygen Priestley found that the gas given off on heating by red oxide of mercury supported combustion better than common air, and, believing this to be due to the fact that it had been deprived of its "phlogiston," he called it "dephlogisticated" air. "Atmospheric air," he argued, "since it is only in part deprived of phlogiston, supports combustion to a limited extent, the process being that the principle of combustion is drawn from the burning body by the air till it is completely phlogisticated. 'Phlogisticated air' does not support combustion at all. Priestley had proved that about one-fifth of common air is not phlogisticated, and this constituent of the atmosphere he was able to identify with the air evolved by the red oxide of mercury. Priestley's discovery, taken up by Lavoisier, led to the refutation of the theory of phlogiston, and to a complete reconstruction of chemical science. Priestley, however, continued to hold the theory to the last, and sought to explain in accordance with it all the new facts discovered by himself and others."

To a rather earlier period, and to Joseph Black, belong also the discovery of the phenomena of "latent heat" and "specific heat."

The name of Henry Cavendish is associated with that of Black as one of the founders of chemical and physical science, which has during the nineteenth century made such remarkable progress

in every department. He showed that oxygen and hydrogen, "dephlogisticated air" and "inflammable air," when mixed in certain proportions and exploded in a close vessel by means of an electric spark, produced pure water, and also that water was formed when a mixture of hydrogen and common air was exploded, and that the diminution in the bulk of the air, about one-fifth, corresponded to the amount of oxygen present in the atmosphere. Thus the proof that water and air were not simple bodies was complete. To Cavendish also is due the discovery of the composition of nitric acid.

Geology.

Towards the end of the last century geologists, who had hitherto been content to collect and describe specimens of rocks and fossils, began to speculate as to the past history of the earth, and in this country the basis of the science was laid by the labours of James Hutton and William Smith. The former, like so many who have advanced and adorned the study of natural science, was educated as a physician, and was the first to endeavour to give fixed principles to geology, which he declared to be in no way concerned about "questions as to the origin of things." "The ruins of an older world," wrote Hutton, "are visible in the present structure of our planet, and the strata which now compose our continents have been once beneath the sea, and were formed out of the waste of pre-existing continents. The same forces are still destroying, by chemical decomposition or mechanical violence, even the hardest rocks, and transporting the materials to the sea, where they are spread out and form strata analogous to those of more ancient date."

The characteristic feature of Hutton's theory was that all past changes on the globe have been brought about by the slow agency of causes which belong to the present order of nature. "In the economy of the world," said he, "I can find no traces of a beginning, no prospect of an end."

The doctrines of Hutton were put forward at a time when the mind of the English public was in a state of feverish excitement owing to the consequences of the French Revolution; they were regarded as an attack upon revealed religion, and were received with fierce opposition. Playfair, his friend and the illustrator of his theory, warmly defended him. "The Author of Nature has

not given laws to the universe which, like the institutions of men, carry in themselves the elements of their own destruction. He has not permitted in His works any symptom of infancy or of old age, or any sign by which we may estimate either their future or their past duration. He may put an end, as He no doubt gave a beginning, to the present system at some determinate period of time, but we may rest assured that this great catastrophe will not be brought about by the laws now existing, and that it is not indicated by anything which we perceive."

To William Smith, an English surveyor, who in 1790 published a tabular view of the British strata, geological science was ultimately indebted for the construction of a geological atlas of the whole of England and Wales. He proved that the order of the strata is never inverted, and that they may be identified at very distant points by the embedded organic fossil remains. Thus the work of generations of naturalists and comparative anatomists, amongst whom John Hunter occupied a distinguished place, was utilised in the construction of the science of palæontology, and proof was afforded of the existence at various periods of many species of animals which are no longer found upon the earth.

To this period also belongs the publication of 'Zoonomia, or the Laws of Organic Life,' by Erasmus Darwin, M.D., F.R.S., which is dedicated "to the candid and ingenious members of the College of Physicians, of the Royal Philosophical Society, of the two Universities, and to all those who study the operations of the mind as a science or who practise medicine as a profession."

Physiology.

The century also witnessed the revival of experimental physiology, and no name stands out more prominently than that of Albert von Haller, who was born in 1708 and died in 1777. The universality of his knowledge is suggested by the fact that at the age of 28 he was appointed professor of anatomy, surgery, chemistry, and botany at Göttingen. Haller refuted the doctrine of the oscillatory motion of the nerves and the theory of the vital spirits which had hitherto been generally held.

He formulated the doctrine of Irritability, which made him famous throughout the world, and he is said to have undertaken

567 experiments, with a view to determine those parts of the body which possess irritability, which he showed to exist in muscular substance alone.

The names of Johannes Müller, Zinn (the Zonule of Zinn), Gasser, the discoverer of the ganglion bearing his name; of Wolff, who described the Wolffian bodies, of Lieberkühn, Meckel, Wrisberg, and Von Sömmering, amongst the Germans; of Valsalva, Santorini, Scarpa, and Morgagni, amongst the Italians; of Winslöw (the foramen of Winslöw), born a Dane, but who lived in Paris, Bichat, Douglas, and Monro—all recall to our memories some part of the body, a knowledge of the minute anatomy of which we owe to their labours, and illustrate the activity of the anatomists of the eighteenth century.

Finally, we come to William Hunter, and to John Hunter, the greatest of them all.

Pathological Anatomy.

In 1761 Morgagni, being then in his seventy-ninth year, published the great work which may be said to have founded the science of pathological anatomy, and before the century closed (1793) Matthew Baillie gave to the world his treatise on 'The Morbid Human Anatomy of some of the most important Parts of the Human Body.'

Medicine.

In Medicine one of the features of the century was the appearance of a number of systems and theories, founded either upon some too slender basis of fact or upon old ideas, which like unto an old umbrella, were "re-covered" and passed off as equal to new—systems which were destined, after a brief struggle for existence, to pass away as rapidly as they arose, and to leave little or no mark upon the future.

The first and perhaps the most important of these was that of Boerhaave, to whom was rightly delivered a letter from a Chinese official, simply addressed "to the most famous physician of Europe." He was probably one of the first to make chemical investigations of the urine, to use a magnifying glass in the examination of the eye, and to employ the thermometer to take the axillary temperature. His reputation as a teacher was so great that he attracted to Leyden students from all parts of the

world in such numbers that no lecture room in the University could contain them.

Boerhaave was an eclectic, and endeavoured to combine what he considered good in all the older systems rather than to put forward one that was entirely new; hence the absence of any guiding principle makes difficult a succinct statement of his system. It is said of him that his medicines were less effective than his personal presence—a statement true of many who preceded him and of many who have followed him, down even to the present day.

The leading idea of the system of Hoffman was the existence of an æther as a dynamic but material principle, "fluid in constitution but very volatile, exerting its action in accordance with the laws of a higher and as yet uninvestigated science, derived in some measure from the atmosphere, but chiefly secreted from the blood in the brain."

Hoffman regarded the medulla as the chief reservoir and centre of the æther, which circulated with the blood and also in the nervous system. Disease consisted in the excessive or diminished influx, the former giving rise to spasm, the latter to atony. Fever, which was due to a general spasm, had its source in the spinal cord. Inflammation was the consequence of a spasmodic arrest of the circulation in the inflamed part.

Whatever we may think of Hoffman's theories his practice appears to have been for the most part sound and his treatment successful. He was fond of prescribing the remedies which he made and from the sale of which he derived a large income; one indeed, "Hoffman's Anodyne," remains in use up to the present day. But his materia medica was not limited to them; he was skilful in dieting, in the use of mineral waters, cold water, bark, and iron. He inspired his Sovereign, Frederick William I, with such confidence in his medical skill that whenever any of the tall grenadiers were ill he invariably sent them to Hoffman for treatment.

Hoffman is said to have caused Peter the Great to wait one whole night to see him, as he made no distinction between his patients. Though great himself he had no exalted idea of the benefits conferred by our profession upon mankind, as may be gathered from the following extract from his writings: "If we compare the good which a half dozen true sons of Æsculapius

have accomplished since the origin of medical art upon the earth with the evil which the immense mass of doctors of this profession among the human race have done, there can be no doubt that it would have been far better if there had never been any physicians in the world."

The system of Cullen took its origin from that of Hoffman in association with the doctrine of irritability put forward about the same time by Haller.

Cullen believed in the existence of some quality pertaining to the nervous system which he termed "nervous force" or "activity" or "the nervous principle" as the life-giving element. This differed from the material æther of Hoffman and the supernatural soul of Stahl.

"An immaterial reasoning substance or soul exists in the living man, and every thought must be considered a faculty of the soul alone. But this immaterial and reasoning part of man is united with the material and corporeal part, and especially with the nervous system, in such a way that the movements called forth in the latter beget thoughts."

Cullen regarded debilitating influences as the chief causes of disease; their effect was to awaken reaction and stimulate the healing power of nature. Cullen's classification of diseases included four classes:—Pyrexiæ, neuroses, cachexiæ, and local affections.

Fevers he divided into intermittent (tertian, quartan, and quotidian) and continued. Amongst the latter are typhus, in which debility predominates; synocha, in which reaction is in excess; and synochus, which is characterised by an admixture of the two foregoing varieties. He was one of the most conspicuous ornaments of the great Edinburgh School of Medicine which became famous in the eighteenth century, and so continued during the nineteenth, and so remains to this day, in spite of many distinguished sons during those two centuries having left it to travel the Great North Road in a direction contrary to that implied by its name, and having found no necessity to return. In his practice Cullen, like Hoffman, was given to simpler measures than were in vogue at the time; he bled less, and made much use of wine, bark, and opium. Of him it has been said:—"It is worthy of notice how valuable a good practitioner is in comparison with a bad theorist, when we distinguish the

therapeutic prescriptions of Cullen from his theoretical explanations." The secret of his success in practice is believed to be the fact that he so often acted in contradiction to his theories.

We cannot leave the systems of the eighteenth century without the mention of that of John Brown—the Brunonian System.

In the first volume of the 'Medical and Physical Journal,' published April, 1799, p. 124, one of the conductors of which was Dr. Thomas Bradley, who on this anniversary in 1800 delivered the Annual Oration before this Society, will be found an account of the chief peculiarities of the Brunonian System. It begins thus :—

"At a time when every University in Europe and America and practitioners in both Indies are directing their attention by encomiums or criticisms to the Brunonian System—when it has begun to be adopted and openly avowed by many popular writers in the kingdom—it may not be thought unreasonable to lay before our readers a concise view of its principles and peculiarities."

Life or the vital principle was then regarded as something separate, distinct, and independent, added to the body at an early period of its existence, there remaining unchanged until as death approached, finding its home no longer tenable it prepared to retire.

The author of this system, on the other hand, regarded life as an assemblage of actions or effects which take place in the body in consequence of a certain predisposition, or of exciting causes, and that the state or amount of the vital principle is not unchangeable but perpetually varying.

The vigour of the system or energy of the vital principle present at any time he called "excitement," that state of the organisation of the solids and fluids of the body which constitutes the predisposition to excitement he termed "excitability"; all those conditions, both internal and external, including medicines, which by acting upon the "excitability" produce "excitement," were named "stimuli."

By the operation of various causes a condition of debility or asthenia might be produced, either directly or indirectly, through the medium of unusual excitement.

In health there is an intermediate grade of excitement; in disease it is too high or too low.

Most diseases were believed to be dependent upon asthenia. Sthenic diseases were those of a truly inflammatory nature.

In order to apply this system in practice the first thing necessary was to determine the "degree of excitability" present in the system, then to so proportion the stimuli as to produce a degree of excitement within the limits of health. Neither careful inquiry into the patient's symptoms nor examination was required, the art of the physician consisted in administering the right dose of the strengthening or weakening remedies.

Opium, ether, wine, exercise, and meat diet belonged to the former class; venesection, emetics, cathartics, fasting, rest, cold, and sweating to the latter.

Sthenic diseases necessarily required weakening remedies, therefore in the treatment of fevers the most important remedies were venesection and abstinence from food.

The students of the University of Edinburgh appear to have regarded the system as a royal road to medicine, and as one which relieved them from the burden of much useless labour.

There is indeed about it an apparent simplicity, but unfortunately it is not always easy to distinguish the "sthenic" from the "asthenic" constitution, and it may be no less difficult to determine whether debility is "direct" or "indirect," and its degree and the kind of "stimulus" which should be applied.

The Professors opposed the new system, but we read that: "this opposition in a University which since the death of Boerhaave has dictated the medical opinions of Europe and America contributed most effectually to disseminate and establish the doctrines of Brown." The students' societies, which had been in the habit of discussing medical subjects on the principles of the spasmodic or Cullenian theory long before the Professors had relinquished the opinions of Boerhaave, now in like manner even during the lifetime of Dr. Cullen adopted the Brunonian System, although the Graduates of the University were not allowed to publish their theses upon any other principles than those taught by the Professors.

Finally, when Erasmus Darwin professed himself to have been a Brunonian even before he had heard of the system, other more or less distinguished men took courage and announced their conversion to the new ideas.

It has been said of the Brunonian System that it sacrificed

more human beings than the French Revolution and the wars of Napoleon combined.

The opening of the eighteenth century was not a brilliant era in the annals of medicine in this country.

In the 'Pharmacopœia of the Royal College of Physicians of London' (1721), such remedies as scorpions, earthworms, woodlice, and vipers' flesh still found a place, and "Mithridatium" contained 49 ingredients.

The original of that famous medicine was, according to Heberden, a very trivial composition, consisting of 20 leaves of rue, one grain of salt, two nuts, and two dried figs, "and he infers that, if even supposing Mithridates ever used the compound (which is doubtful), his not being able to despatch himself was less owing to the strength of his antidote than to the weakness of his poison."

It is difficult for the physician of to-day to realise how forlorn would be his condition if deprived of his stethoscope and clinical thermometer, of his microscope and ophthalmoscope, of his knowledge of the regional anatomy of the brain and spinal cord, and of the information which he obtains from the chemical examination of the urine and other secretions.

Pondering on it, I am reminded of the remark of a friend who inquired the subject of my Oration. I answered, "Science and Medicine in the Eighteenth Century." He replied, "Was there any then?" Possibly the *tactus* was more *eruditus*, and the advice given on such subjects as diet and regimen quite as wise then as to-day, and less swayed by passing theories and fads which rightly bring upon us the scorn of unbelievers. The physicians of that time, if not profound in medical lore, were, at any rate, remarkable for their classical scholarship. John Arbuthnot, the favourite physician of Queen Anne, the friend of Pope and Swift, with whom he wrote the first book of the 'Memoirs of Martinus Scriblerus,' is described by Dr. Johnson as "a man of great comprehension, skilful in his profession, versed in the sciences, acquainted with ancient literature, a scholar with great brilliance of wit, a wit who in the crowd of life retained and discovered a noble ardour of religious zeal." He is said to have possessed "all the wit of the Dean (Swift) without his virulence and indelicacy, and a considerable proportion of the genius of Pope without his querulous discontent."

Radcliffe, famous for his benefactions to the University of Oxford, and Richard Mead, the successor of Radcliffe in his house, his practice, and in the possession of the celebrated gold-headed cane, were also leading physicians of this time.

Mead was an accomplished scholar, a patron of the fine arts, a great collector, and the friend of the most distinguished literary men of the day. At his table were seen the most eminent men of the age, both natives and foreigners, and he was often the only individual present who was acquainted with all their different languages. He it was who persuaded the wealthy citizen, Guy, to bequeath his fortune towards the foundation of the noble hospital which has honourably consecrated his name. Medicine has advanced since those days, and we should not now expect from one of its leaders, 'A Treatise concerning the Influence of the Sun and Moon upon Human Bodies and the Diseases thereby Produced,' but few of them have since loomed quite so large in the public eye as did Richard Mead.

The practice of his profession brought him wealth, which he spent liberally, but to medicine he contributed comparatively little. He published a discourse concerning pestilential contagion, in which he warmly supported the true view that the plague was a contagious disorder, and advocated the removal of sick persons from their homes and the formation of a cordon around infected places. He supported Lady Mary Wortley Montague in the introduction of the practice of inoculating for small-pox, and also published a treatise on that disease and measles. To Mead we owe the suggestion that a bandage should be applied to the abdomen during the operation of tapping for ascites.

John Freind, the son of a Northamptonshire clergyman, was the most learned of all the scholarly physicians of this period. He turned to the study of medicine with the same ardour which he had already shown in the cultivation of the classics. For two years he filled the post of physician to the army, an appointment which recent events have shown might well be revived, for war brings sickness as well as wounds, and the sick need a physician. With Dr. Mead he was on terms of intimate friendship, and the story has often been told how the latter, when Freind had been committed to the Tower for supposed participation in the Bishop's plot, refused to prescribe for

Sir Robert Walpole until the Minister had signed an order for Freind's release.

His most celebrated work is 'The History of Physic from the Time of Galen to the Beginning of the Sixteenth Century, chiefly with Regard to Practice, in a Discourse written to Dr. Mead.'

Dr. Freind, who was then a member of Parliament, presented to the House of Commons, in 1725, the Petition of the College of Physicians on the pernicious and growing use of spirituous liquors. He died in 1728, aged 52 years.

I must pass by Askew and Pitcairn, who in succession carried the gold-headed cane.

William Heberden, who lived during 90 years of the century and saw the beginning of its successor, was distinguished at the University of Cambridge, and still more so in later years in London. He so lived his life as to acquire "the love and esteem of all good men"; than which there can, surely, in this world be nothing better worth possessing. He was the friend and physician of Samuel Johnson, who often refers to him in his letters in terms of affection and esteem, and who dubbed him, "Ultimus Romanorum, the last of our learned physicians."

Let me digress in order to present to you briefly a clinical case, described in the patient's own words. It is a picture of the medical practice of the time.

The case of Samuel Johnson, LL.D., æt. 74 years, date June 17th, 1783. Physician Dr. Heberden. *Previous History.*—"I have been accustomed to bleed frequently for an asthmatic complaint, but have forborne sometime by Dr. Pepy's persuasion, who perceived my legs beginning to swell. I sometimes alleviate a painful or, more properly, an oppressive constriction of my chest by opiates, and have lately taken opium frequently, but the last or two last times in smaller quantities. My largest dose is three grains, and last night I took but two." June 16th.—"I went to bed, and in a short time waked and sat up . . . . when I felt a confusion and indistinctness in my head, which lasted, I suppose, about half a minute. I was alarmed, and prayed God that however he might afflict my body he would spare my understanding. This prayer, that I might try the integrity of my faculties, I made in Latin verse. The lines were not very good,

but I knew them not to be very good. I made them easily, and concluded myself to be unimpaired in my faculties. Soon after I perceived that I had suffered a paralytic stroke, and that my speech was taken from me. I had no pain and so little dejection in this dreadful state that I wondered at my own apathy. . . . . In order to rouse the vocal organs I took two drams. Wine has been celebrated for the production of eloquence. I put myself into violent motion . . . . but all was vain. I then went to bed, and, strange as it may seem, I think, slept. When I saw light it was time to contrive what I should do. Though God stopped my speech he left me my hand . . . . my first note was necessarily to my servant, who came in talking, and could not immediately comprehend why he should read what I put into his hands. . . . . I then wrote to Dr. Taylor (the Rev. Dr. John Taylor) to come to me and bring Dr. Heberden. . . . . My physicians are very friendly and give me great hopes. . . . . June 18th.—I have so far recovered my vocal powers as to repeat the Lord's Prayer with no very imperfect articulation. My memory, I hope, yet remains as it was, but such an attack produces solicitude for the safety of every faculty. Sickness is, by the generosity of my physicians, of little expense to me."

Diagnosis.—Thrombosis of a branch of the left middle cerebral artery.

The *treatment* is not fully reported. "The physicians came and gave the directions which the disease required."

July 1st.—"I had leave . . . . to wash the cantharides from my head. I took an airing to Hampstead and dined with the Club, where Lord Palmerston was proposed and, against my opinion, rejected."

He completely recovered from this attack, and a year later (July 20th, 1784) sent the following account of his condition for Dr. Heberden :—

"Tell Dr. Heberden that in the coach I read 'Ciceronianus,' which I concluded as I entered Lichfield. . . . . The asthma has no abatement, opiates stop the fit . . . . but they do not procure me the power of motion, and I am afraid that my general strength of body does not increase. The weather, indeed, is not benign, but how low is he sunk whose strength depends upon the weather. I am now looking into Floyer, who lived with his asthma to

almost his 90th year. His book, by want of order, is obscure, and his asthma I think not of the same kind with mine, and now, *abite curæ*, let me inquire after the Club."

A little later he writes:—"Better, I hope, and better. . . . . I am favoured with a degree of ease in breathing that very much delights me, and do not despair of another race upon the stairs of the Academy. . . . . The water rises, though it does not rise very fast. The remission of one disease will enable Nature to combat the rest. . . . . The squills I have not neglected. . . . . If the virtue of medicines could be enforced by the benevolence of the prescriber, how soon I should be well! I never thought well of Dr. James's compounded medicines (of fever powder fame), his ingredients appear to me sometimes inefficacious and trifling, and sometimes heterogeneous and destructive of each other. This prescription exhibits a composition of about three hundred and thirty grains, of which there are four grains of emetic tartar and six drops (of) thebaic tincture. He that writes thus surely writes for show. The basis of his medicine is the gum ammoniacum which dear Dr. Lawrence used to give, but of which I never saw any effect. We will, if you please, let this medicine alone. The squills have every suffrage, and in the squills we will rest for the present." Obiit, December 13th, 1784, aged 75 years.

The autopsy revealed the presence of gout, granular kidneys, and emphysema. The drawing of an emphysematous lung in Dr. Matthew Baillie's work on 'Morbid Anatomy' was taken from one of the lungs of Dr. Johnson.

Dr. Heberden, when over 70 years of age, began to compile the 'Commentaries on the History and Cure of Diseases,' a work which will keep his memory ever green, though fingers, as time rolls on, should cease to be deformed by nodes. The work was published after his death.

Does there exist in the history of medical literature another instance of an author who has secured a place amongst the immortals by a posthumous work? If not, it is, I fear, hopeless to look to our time to produce one.

Matthew Baillie, born in 1761 in a Scotch manse, a home of a modest kind, from which there has gone forth into the world probably more men who have achieved distinction than from any

other in these Islands, was the nephew of William Hunter, his assistant in his lectures, demonstrator in the Anatomical School in Great Windmill Street, and, jointly with Mr. Cruikshank, his successor there upon the death of William Hunter in 1783. From 1787 to 1799 he was physician to St. George's Hospital, but in the latter year he resigned his appointment and lectureship in order to devote his time entirely to private practice, in which he was so successful that his income is said to have reached during several successive years the sum of £10,000.

In the eulogium of Dr. Baillie pronounced by Sir Henry Halford as President of the College of Physicians, upon his death in 1823, we see very clearly the change which he introduced into the spirit of medical practice. "Before his time it was not usual for the physician to do much more than prescribe remedies for the malady, and encourage the patient by such arguments of consolation as might present themselves to humane and cultivated minds. But as the assumed gravity and outward signs of the profession were now considered obsolete customs, and were by general consent laid aside by the physicians, and as a more curious anxiety began to be observed on the part of the patient to learn everything connected with his complaint, arising naturally from the improved state of general knowledge, a different conduct became necessary in the sick room. . . . . He appeared to lay a great stress upon the information which he might derive from the external examination of his patient, and to be much influenced in the formation of his opinion of the nature of the complaint by this practice. He had originally adopted this habit from the peculiar turn of his early studies, and assuredly such a method, not indiscriminately but judiciously employed, as he employed it, is a valuable auxiliary to the other ordinary means used by a physician of obtaining the knowledge of a disease submitted to him. But it is equally true that, notwithstanding its air of mechanical precision, such examination is not to be depended upon beyond a certain point. Great disordered action may prevail in a part without having yet produced such disorganisation as may be sensibly felt, and to doubt of the existence of a disease because it is not discoverable to the touch is not only unphilosophical but must surely, in many instances, lead to unfounded and erroneous conclusions."

At the end of the nineteenth century, a period which has been

mainly characterised by the eagerness and success with which every advance in the sciences allied to medicine has been utilised for the diagnosis of disease, it appears strange that any doubt could ever have existed as to the importance of the information to be derived from an examination of the patient, but so it was. Herein we seem to observe the beginning of modern scientific medicine. The gold-headed cane, the full-bottomed wig, and the assumed gravity disappear, and in their place we have the observer trained in the methods of science, examining by their aid the condition of the various organs of the body and forming a diagnosis, not by the exercise of some mysterious power but upon a careful weighing of the evidence thus obtained. When will the British public learn that this is the only true method in medicine?

Matthew Baillie evidently appreciated that in the practice of medicine diagnosis comes before all else; once this is clear the lines on which treatment should proceed are not in most cases difficult to find, we know at least what can and what cannot be done. He had prepared himself for the practice of his profession as a physician by a prolonged study of morbid anatomy, which must ever provide the most sure groundwork of knowledge, a study which, so to speak, removes the coverings of the internal organs and enables us to form mental pictures of their condition in disease. Medicine will continue to call to its aid every discovery in science which has a bearing upon the prevention, diagnosis, or treatment of disease, but for these to be successful a knowledge of the cause and of the changes which it produces is, speaking generally, essential, and in the future, as in the past, it is to pathology that we shall be indebted for all the great advances in medicine.

John Lathom was another distinguished London physician of this period. He was successively physician to the Manchester Infirmary, the Radcliffe Infirmary at Oxford, the Middlesex Hospital, and St. Bartholomew's Hospital. He delivered the Gulstonian Lectures in 1793 at the College of Physicians, the Harveian Oration in 1794, and the Croonian Lectures in 1795, and was subsequently for seven years (1813–1819) President of the College. In a memoir of him by his son it is said that "he was singularly temperate when temperance was hardly yet thought to be a virtue, he was most pure in life and conversation

when to have been otherwise would have provoked no censure, and he was not ashamed to be religious when religion had yet no recommendation or countenance from the world."

Sir George Baker, a distinguished scholar and physician and for nine years President of the College of Physicians, is best known for his essays on the cause of the colic of Devonshire and Poitou, which he clearly traced to the presence of lead in the system.

In the year 1800 the President of the Royal College of Physicians was Dr. Thomas Gisborne, Fellow of St. John's College, Cambridge, Physician in Ordinary to the King, and for 24 years Physician to St. George's Hospital. He was probably a physician of the academic type, the existence of which so excited the animosity of a certain medical reformer of those days, by name Jeremiah Jenkins, Esq., that writing somewhat later (1810) he delivered himself thus :—

"This diffusion of a knowledge of medicine has proved fatal to its mysteries and charms. The public has long discovered that the wig, the cane, and a knowledge of the dead languages are not necessary to form the practical physician. They are also aware that it is highly necessary that a physician should be well versed in the different branches of the profession to be entitled to their implicit confidence. Hence we find that the physicians who have been principally educated in the School of Surgery and Pharmacy carry nearly the whole practice of London, viz., Dr. Baillie, Dr. Babington, Sir Walter Farquhar, and Dr. Dick. It being a well-known fact that Oxford and Cambridge are no longer schools for medicine, the physicians who have been solely educated at those Universities enjoy a very small proportion of the public confidence. A little reflection on the subject must convince one that the practitioner who has been educated in the schools of London, Edinburgh, or Dublin, whether he has a diploma or not, is more entitled to his confidence than the person who has led a life of dissipation at either of the English Universities."

There were apparently "lost medical schools" even in 1800, fortunately they have since been found.

George Fordyce, who was born in Aberdeen in 1736, and died in Essex Street, Strand, in 1802, was a favoured pupil in Edinburgh

of Dr. Cullen, and later of Dr. William Hunter in London. At the age of 28 he began to lecture in London on materia medica and the practice of physic. He also lectured on chemistry. Whether before his time or since there has ever been so prolific a lecturer I know not. Few in the present day would venture to emulate such a record as he possesses. The three subjects just mentioned he continued to teach with rapidly increasing reputation for nearly 30 years, giving for the most part three courses of lectures on each subject in every year. A course lasted nearly four months, and during it a lecture was delivered six times in the week. His time of teaching commenced about seven o'clock in the morning and ended at ten o'clock, his lectures on the three subjects being given one immediately after the other. Eight hundred and sixty-four lectures in one year; what a prodigious outpouring!

Dr. Fordyce was a Fellow of the Royal Society and of the Royal College of Physicians, and also a member of the Literary Club, a distinguished body founded by Dr. Johnson, and which during his lifetime included amongst its members Sir Joshua Reynolds, Burke, Topham Beauclerk, Bennet Langton, Goldsmith, Sir John Hawkins, Boswell, Bishop Percy, Garrick, Fox, Gibbon, Adam Smith, Sheridan, and Lord Stowell.

"Dr. Fordyce," we read, "was always fond of society, and in the earlier years of his life, to render the enjoyment of its pleasures compatible with his professional pursuits, he used to sleep but little. He was often known to lecture for three consecutive hours in the morning without having undressed himself the preceding night. He had satisfied himself that man eats far oftener than nature requires, and for many years, like the hawk and the hound, he took but one meal in the 24 hours. He dined every day for more than 20 years at Dolly's chop-house in Paternoster Row. At four o'clock the doctor regularly took his seat at a table always reserved for him, on which were placed a silver tankard of strong ale, a bottle of port wine, and a measure containing a quarter of a pint of brandy. The moment the waiter announced him the cook put a pound and a half of rump steak on the gridiron, and on the table some delicate trifle as a *bonne bouche*, to serve until the steak was ready. This was sometimes half a boiled chicken, sometimes a plate of fish; when he had eaten this he took one glass of brandy, and then proceeded

to devour his steak. When he had finished his meal he took the remainder of his brandy, having during dinner drank the tankard of ale and afterwards the bottle of port. He thus spent an hour and a half of his time, and then returned to his house in Essex Street. He made no other meal until his return next day at four o'clock to Dolly's. The vigour of his constitution enabled him to sustain for a time without apparent injury this mode of life. But at length he was attacked with gout, which afterwards became irregular."

There is, I think, a certain grim humour about the use of that word "irregular." It was a scurvy trick of the disease thus to become as it were "dissipated" in one who had always led such a regular life.

We are told of him that "his manners were less refined and his dress in general less studied than is expected in this country in the physician. From these causes, and from his spending no more time with his patients than was barely sufficient for forming a just opinion of their ailments, he had for many years but little private employment in his profession, and never, even in the latter part of his life when his reputation was at its height, enjoyed nearly so much as many of his contemporaries."

James Carmichael Smyth was another member of the distinguished Highland band who at this time moved towards the south. He was born in Perthshire in 1741, educated in Edinburgh, and settled as a physician in London in 1768, being elected a physician to the Middlesex Hospital in 1775. In 1780 he was appointed by the Government to take charge of the prison and hospital at Winchester, where a malignant form of typhus fever was raging amongst the Spanish prisoners. He employed the fumes of nitrous acid to destroy the contagion, and this measure proved so successful that he was voted a sum of £5,000 by Parliament as a remuneration of his services. He was also appointed Physician Extraordinary to the King, and elected a Fellow of the College of Physicians. He wrote 'An Account of the Effects of Swinging Employed as a Remedy in Pulmonary Consumption.' I presume that this treatment was to be conducted in the open air.

Thomas Bradley cannot be included amongst the leaders in medicine at the close of the last century, but as the Orator of this Society in 1800 he deserves mention at my hands. He began life

as a schoolmaster, and is said to have been highly proficient in mathematics. In 1786, however, he forsook this pursuit, and five years later graduated in medicine at the University of Edinburgh. Settling in London, he became, in 1794, physician to the Westminster Hospital. It is said of him that "his retired habits in early life had unfitted him for the metropolis, to which he proved unequal rather from diffidence than from want of professional knowledge. He was more read in books than in men, and as he always hesitated, like a genuine mathematician, to draw conclusions from uncertain premises he appeared to little advantage in the sick room." He was evidently of the type of a distinguished physician whom we have all met, who, having been summoned to a consultation at a distance in the country, is reported to have informed the friends that the patient was suffering from one of three diseases, but he felt unable to determine which. It is said that they regretted the expense of his visit.

Surgery in the Eighteenth Century.

During the first half of the eighteenth century the union which had been formed in 1540 between the Guild of Surgeons and the Guild of Barbers, under the title of the United Company of Barber-Surgeons, still existed. Of the origin of the Guild of Surgeons no record exists, but Mr. D'Arcy Power tells us that the first notice of this body is in 1369, when "four surgeons were admitted at full hustings before the mayor and aldermen of London, and were sworn as master surgeons to deserve well and truly of the people in attending their cases, to take only reasonable payment of their patients, to practice their craft, and report as often as need be to the mayor and aldermen the faults of those who undertook the practice of surgery."

Highly as we may reverence the ancient Corporation of the City of London, we should not, I think, nowadays regard it as the most suitable court to revise the faults of our surgical colleagues.

The origin of the barber-surgeons is also veiled in the obscurity of the past.

The subsequent history of the two bodies which, after many years of strife, were thus united has been so recently and pleasantly told by Mr. D'Arcy Power that I forbear to enter upon

it. The union lasted for over two centuries, and during that period amongst the members of this double-headed craft there were, doubtless, many like Gale, Clowes, Halle, Read, and Banister, who laboured honestly and with high purpose to advance the art of surgery, which in the time of Gale was "accounted so beggarly and vile a calling that no man who had well brought up his son would apprentice him to it."

Few of them, however, have left their mark upon its annals. That they were held in such low esteem was doubtless in part due to their association with the barbers, partly also to the opposition of the physicians, who in 1632 obtained an order in Council that "no chirurgeon doe dismember, trepan the head, open the chest or belly, cut for the stone, or do any great operation with his hand upon the body of any person to which they are usually tied to call the warden and assistants, but in the presence of a learned physician, one or more of the College or of Her Majesty's Physicians."

Although at a later period, in such men as Woodall (1569–1643), and Wiseman (1623–1676), the art of surgery found worthy exponents, it was not until the eighteenth century is reached, and coincident with the increase in number and importance of the hospitals, that we find the surgeons tentatively asserting their right to lecture and teach independently of the Company.

Cheselden, the "first of the great hospital surgeons in London," when summoned before the Court of Assistants of the Company of Barbers and Surgeons in 1714, for having procured the dead bodies of malefactors and carried on dissections at the same hours as the Company's lectures, did, it is true, submit himself to the pleasure of the court and promise not to offend again in like manner, but it is clear that the Company was jealous of Cheselden because his lectures were more attractive than those delivered in the Barber-Surgeons' Hall.

To Cheselden is due the chief credit for having brought about the divorce which severed the degrading alliance between the useful but humble trade of barbery and the noble art of surgery. During the latter half of the century by the labours of Alexander Monro primus, Samuel Sharp, Perceval Pott, and above all John Hunter, surgery secured its rightful place as a pursuit worthy of the highest intelligence and as the calling of a gentleman.

The Surgeons Company, which was established by Act of Parliament in 1745, continued to exist for just over 50 years, but it effected little for the advance of science and was apparently unmourned upon its decease in 1796.

Four years later the College of Surgeons in London was created by royal charter, the Institution which, as the Royal College of Surgeons of England, will this year celebrate the centenary of its foundation, and will then be able to look back with satisfaction and pride upon a hundred years, during which it has achieved and worthily maintained the highest position amongst the licensing bodies in the United Kingdom, has striven to advance the science and art of surgery, and has proved itself a faithful custodian of that great museum which will ever remain the most fitting monument of the genius of John Hunter.

Medicine has indeed advanced in the past century, but surgery has been born again, we scarce indeed can recognise in its new and cleanly garb the art which even in my student days so often led its subjects by a short cut to the realms of pathological anatomy.

When looking back to those times and thinking of the brilliant operators whom, as house surgeon, I have assisted, and of the awful results of their handiwork, results about which, in the aggregate, nobody cared, I have often literally shuddered. Not being as yet quite an old fogey you will scarcely credit that I have seen maggots in a wound in the wards of a London hospital, and that no one thought much of it. I remember on one occasion informing a surgeon that six of his cases had developed erysipelas, not an unusual number, when he replied: "Ah, I've often known the wounds do better after erysipelas."

Then the great object of the surgeon in operations in the neighbourhood of the abdomen was to keep clear of the peritoneum, now he lives but to divide it.

Of the many Societies now connected with the profession the only one, apart from the Hospital Societies, existing in the year 1800 was that which to-night I have the honour to address. The meetings were then held at the Society's house in Bolt Court, Fleet Street, and the President was Dr. James Sims. He was the first elected to the chair in 1786, and continued in the presidency until 1809, when he was succeeded by Dr. John Coakley Lettsom, whose name in this Society will always be mentioned with honour.

A matter in which the profession and this Society were greatly interested at the end of the last century still, to the eternal disgrace of the people of this country, remains, here but nowhere else in the world, a burning question in 1900.

On Monday, June 18th, 1798, "The President, Dr. Sims, related some circumstances lately observed in a disease incident to cows, called the cow-pock, which, when communicated to the human species, is said to remove the liability to small-pox in persons who have never had that disease. These observations have been made by a Dr. Jenner, of Cheltenham, who is about to publish them."

At many subsequent meetings the minutes state that "the evening was employed in conversation on the cow-pox." So important was it considered that at one meeting it was even "resolved that the subject of the atmospheric air be considered on some future occasion."

On April 29th, 1799, "a letter from Dr. Jenner, of Gloucestershire, respecting the cow-pox was read, which subject engaged the Society for the evening." On April 28th, 1800, Dr. Jenner attended a meeting of the Society as a visitor and presented a copy of his work, entitled an 'Inquiry into the Causes and Effects of the Variolæ Vaccinæ,' edition second, 4to, London, 1800, for which the Society's thanks were made by the President. At this meeting Mr. Astley Cooper, surgeon, was elected a member of the Society, and the evening was engaged in conversation on the cow-pox. The minutes, alas! fail to tell us what Dr. Jenner said on that occasion.

That the members of this Society showed an intelligent appreciation of the value of Jenner's work is proved by the following entry, on March 29th, 1802:—"Resolved that the members of the Medical Society of London (having) taken into consideration the very important discovery made by Dr. Edward Jenner, are of opinion that great benefit will accrue to the inhabitants of these islands and to mankind in general from the introduction of vaccine inoculation, and that from their own experience as well as from the extensive and successful trials made in various parts of the world, it will in all probability ultimately eradicate the small-pox, one of the most fatal diseases to which the human species is liable.

"Resolved that a copy of this resolution signed by the President be presented to Dr. Jenner."

At the anniversary meeting on Thursday, March 8th, 1804, which was numerously attended, the President made an appropriate apology for the absence of Dr. Sayer Walker, in consequence of severe indisposition, which had prevented him from preparing or delivering the Annual Oration. Dr. Lettsom then favoured the Society with an Oration "On the Origin of Vaccine Inoculation," accompanied by a biographical account of Dr. Jenner, after which the Orator presented the gold medal (which had been previously voted by the Society to Dr. Jenner) into the hands of Dr. Sims, who had been appointed to receive the same for Dr. Jenner. After which Dr. Lettsom gave the Society a second Oration on the subject of the "Yellow Fever in the West Indies." Let me at once assure you, Sir, and the Society, that on the present occasion I have no thought of following the example then set by Dr. Lettsom.

When one reviews the antagonism during the past century of a noisy but still considerable section of the people of this country to the practice of vaccination, culminating as it did in the recent legislation on the subject, and at the same time has regard to the position which as a people we hold in the world, one's admiration for the force of the national character should be unbounded. Surely there must be something very great in a people which by their action on this and other scientific questions have shown themselves to be such fools! Or would it not perhaps be wiser to bear in mind that this agitation has hardly appeared in Scotland or Ireland, and that the character which we are too apt to call English, and which in peace and in war has raised this country to its present position, is really British, and consists in the happy union of the national qualities of the English, the Welsh, the Scotch, and the Irish.

I venture to express the opinion that the recent Vaccination Act will really prove a blessing in disguise; its framers have, without the intention of so doing, brought the importance of the matter home to every household in the country, and it is no longer "a matter for the doctors." In time the ratepayer will find that to vaccinate costs less than to keep a patient with small-pox; the dwellings and the avenues in life open to the unvaccinated will, I believe, gradually be diminished in number, and out of evil good will come.

When the next century shall have nearly passed away, and on this anniversary, in the year 2000, the Orator of the day reviews, as possibly he may, the changes it has brought, and speaks, perhaps in scarcely veiled scorn, of the ignorance of our time, I trust that he will find this Society, though older, still not aged, vigorous as now but wiser with the lapse of years, and that he may be honoured with an audience as patient as that which I have had the pleasure to address.

CLINICAL EVENINGS.

November 13*th*, 1899.

TWO CASES.

By Edmund Cautley, M.D.

(1) *Aphasia.*

The patient, a tinman, aged 71, had been a healthy man all his life. On April 18th he had a fit, with convulsive movements on the right side, and unconsciousness for three-quarters of an hour. On recovering consciousness, there was aphasia with some loss of power on the right side. When he came under observation, on May 23rd, he was suffering from aphasia and agraphia, but there was no trace of paralysis. The aphasia was of the type known as motor aphasia; it was not quite complete, as the patient could utter a few words, though not what he wanted to say. The urine had a specific gravity of 1012, and contained a trace of albumin. The agraphia was well marked as late as the end of July, but had now been recovered from. The aphasia was decidedly less, though still well marked. There was no impairment of intelligence. The diagnosis put forward was that of hæmorrhage into the second and third left frontal convolutions, anterior to the centres for speech and writing, which had been compressed, and the former, to a certain extent, damaged.

(2) *Congenital Heart Disease, probably Aortic.*

Dr. Cautley also showed a lad, aged 14, the second of seven children, who were all living. There was no family history of rheumatism. Though a "fine baby born," he soon wasted. There was no history of lividity or fits at birth. He had measles as a baby, a fit at 2 years, and diphtheria at 9 years of age; never scarlet fever or rheumatic affection. In other

respects he had had good health and only came under observation for an attack of diarrhœa.

The boy was very small for his age and rather high-coloured. Pulse 72, rather slow. The heart apex was in the fifth space in the nipple line; there was no increase of dulness to the right, and only a very small increase to the left. Over the chest, above the level of the fourth rib, was a very marked systolic thrill extending outwards to the right and left, and into the vessels in the neck. Corresponding to the thrill was a loud systolic murmur. The thrill and murmur were best marked in the first and second intercostal spaces to the right of the sternum. The murmur could be heard over the back, and a short systolic murmur, probably the same, could be heard at the apex and in the axilla. Although cases of congenital aortic stenosis were rare, he thought that this was an instance, judging from the history of the case and the distribution of the thrill and murmur.

The PRESIDENT agreed that it was probably a case of congenital disease, and asked whether there was any history of any other congenital malformation or taint—syphilis, for example.

Dr. C. W. CHAPMAN thought he heard a mitral bruit distinct from the aortic sound. He thought it was a case of much noise without there being very much wrong, judging from the small effect the lesion had on the heart and the general health.

Dr. CALVERT thought there was a good deal of obstruction, as evidenced by the great hypertrophy of the left ventricle. He pointed out that the pulse was decidedly small, and this was also suggestive of marked stenosis. He suggested that a sphygmographic tracing of the pulse would throw considerable light upon the nature of the lesion. He considered it to be a case of aortic stenosis, which, as a congenital occurrence, was extremely rare.

Dr. EWART thought that the second sound of the aorta was rather weak, and suggested, as there was no reflex murmur, that possibly not much blood passed that way. There was a loud murmur in the aorta, which was audible for a considerable distance down the back. It was conceivable that there was a permanent ductus arteriosis, which was known to give rise to very loud murmurs. These murmurs were loudest in the manubrial region and to the left of the sternum, and were conducted down the aorta. Some of these features were present in this case, but there was not the double murmur which he regarded as diagnostic of the lesion in question.

Dr. CARR observed that the thrill and murmur were so well felt and heard to the left as well as to the right of the sternum that it was evident that the blood was thrown into vibration in the pulmonary artery as well as in the aorta. He suggested that there was a communication giving rise to a disturbance in the current in both vessels, but so small as not to cause any cyanosis or obstruction, there being very little indication of appreciable hypertrophy of either ventricle.

The PRESIDENT called attention to the absence of any history of cyanosis.

Dr. CAUTLEY, in reply, observed that the fact that there were seven living children and no miscarriages seemed to contra-indicate syphilis. He did not think that there was much hypertrophy, which, such as it was, was limited to the left ventricle. The boy was a small boy, and had a small heart, in proportion to his size, showing that the lesion did not seriously affect his health, though it interfered with his development. He admitted that it was mere speculation as to the exact nature of the lesion or lesions, for there might be more than one. The absence of any history of cyanosis was, he thought, against the idea of a communication with the pulmonary artery, and a lesion of the pulmonary orifice was contra-indicated by the absence of evidence of hypertrophy of the right ventricle. He thought the main lesion, if not the only one, was aortic stenosis.

NOTE.

The patient in this case died suddenly eight days after being shown, and at the meeting of the Society, on the 12th February, 1900, Dr. Cautley exhibited the heart, which weighed 5½ ounces, and was evidently hypertrophied, as proved by the thickness of the wall of the left ventricle. The aortic valves were very much thickened, rigid, and adherent at their edges, with the exception of a small, irregular, slit-like orifice between two of the valves. No other abnormality was present. The diagnosis of aortic stenosis was, therefore, correct, though the congenital origin of the affection could not be proved.

TWO CASES.

By WILLIAM EWART, M.D., F.R.C.P.

(1) *Persistent Lockjaw in an Edentulous Subject relieved by Incising a Tender Portion of the Gum.*

HENRY S., aged 47, decorator, an edentulous, relatively senile-looking man, with slight risus and stiff neck and anxious expression, was admitted into St. George's Hospital, after a three weeks' illness, on September 20th, 1899. He gave no history of any traumatic lesions, except a fall on the head several years ago. He had not suffered much from illness, had not had syphilis, but admitted being a hard drinker up to the onset of his illness. There was a large chronic ulcer of the right leg, with a dirty

sloughing base extending partly over the back as well as the front of the leg. He was unable to speak intelligibly, owing to the permanent closure of the mouth and absence of teeth. The illness began with a cold. A fortnight before admission he woke in the night with suffocation, and found himself unable to open his mouth, and was obliged to separate his lips with his fingers to allow air to enter. Since then the condition of lockjaw has persisted. He said that the neck has been stiff only for the last day or two.

Besides the slight risus sardonicus, affecting chiefly the orbits, and narrowing the palpebral aperture, there was much stiffness of the neck without retraction of the head, and anteriorly the platysma was strongly contracted, its pectoral fibres showing up half-way down the chest. The lips were not easily separated, and caused frequent obstruction to the breathing, particularly in the night. There was also much stiffness and discomfort at the floor of the mouth from spasm. The nocturnal attacks of suffocation were explained by his being unable, according to his own statement, to breathe through the nose during the seizures, perhaps owing to superadded spasm of the levator palati and other muscles. The abdominal muscles were "board-like," but there was no opisthotonos and no rigidity or spasm of the limbs. Knee jerks increased. In addition to the tonic contraction, excitement produced tighter closure of the jaws, and an increased contraction of the muscles might be noticed in the fibres of the platysma.

The patient complained of no severe pain, except a throbbing headache over the vertex arising from the occipital region. He had lost much flesh during the last few weeks, owing to the mechanical difficulty of alimentation. Pulse 64, tension good; respiration 34; tongue furred, bowels open; the urine contained no albumen.

Progress and Treatment.—When the contraction and tenderness had been relieved by ice massage applied to the maxillary region sufficiently to render possible an examination of the gums, it was found that at the left upper molar region, though no underlying stump could be felt, there was great tenderness on pressure, extending also up the posterior border of the alveolar process. Ice massage, which was used at intervals for a day or two, led to a slight diminution in the tightness of closure of the

locked jaw, but the improvement was temporary only. The inhalation of carbonic acid was also tried, but did not relax the contraction in any appreciable degree. The condition remained practically unrelieved, and the nocturnal attacks continued to recur until surgical measures were adopted.

The case had been diagnosed from the first as one of reflex spasm from periosteal irritation and tenderness at the left posterior extremity of the upper jaw, and local treatment had been recommended, but not carried out until the uselessness of internal medication by various sedatives and by biniodide of mercury had been demonstrated. This was finally resorted to, and the symptoms rapidly disappeared after the tender gum had been freely incised. Mr. H. L. Albert, who operated, found no lesion or abnormality. The tenderness was almost immediately removed, and the lockjaw was completely relieved, and has not since then returned.

The case was a good illustration of the reflex tetanoid symptoms from irritation of the dental nerves occasionally reported. These cases did not present the complete symptoms of traumatic tetanus, they did not end fatally, and they were invariably cured by a removal of the offending cause. The few cases of fatal tetanus which had been observed after operations on the teeth were probably cases of accidental infection with the bacillus.

References are given in the 'American System of Dentistry,' p. 489 *et seq.*, to three instances of fatal tetanus occurring after dental operations, and to several instances of the minor or tetanoid group, to which the present case belongs.

The case exhibited is a perfect instance of a purely reflex causation due to nerve irritation, since, owing to the absolutely edentulous and clean condition of the gums, the mouth contained no source of septic infection.

(2) *A Case of Pigmentation of the Skin and of Partial Pigmentation of the Nails, simulating Addison's Disease, in an Asphalt Worker, and of Anæmia associated with Hepatic and Splenic Enlargement.*

Antonio M., aged 43, an Italian born in England of healthy, long-lived parents (he stated that his father died aged 89 and mother aged 101), had been out of health since last spring, when

he was laid up in bed for seven weeks by a severe attack of influenza followed by great loss of weight and of strength.

His previous history was as follows:—He was taken to Italy when 2 years of age, where he lived until he was 8 years old, and then returned to London. He remembered no illnesses during this period, except a scald in left hypochondriac region, signs of which were present. He lived with his parents in London until he was 14 years old, when he had an attack of "yellow jaundice," for which he was treated at St. Bartholomew's Hospital. At 15 he became a cabin-boy on board a ship, and visited Venice, Constantinople, and New York. He also sailed along the coast of India, but never landed. He travelled about for four years and then returned to London. He had no illness while at sea. On returning to London at the age of 19 he started asphalting in the streets, and this occupation he had followed ever since. He worked for the Fulham Vestry for eight years, and was then transferred to Rotherhithe, where he superintended the cutting up of the wooden blocks for the street pavement. Whilst working there in the winter he caught a severe cold with bronchitis, and his cough had returned every winter since.

When 23 years of age, and during the time he was working for the Fulham Vestry, he went to Naples for a fortnight to see his parents. It was a very hot summer and, though previously pretty fair in complexion, he became very dark (sunburnt), and had retained that colour ever since.

He had worn a truss for the last 25 years. The dark colouration of the skin in the region of the truss girdle had been noticed for the last 12 or 13 years.

He had suffered from hot sweats at night for the last eight years, but had been able to sleep regularly from 8 p.m. to 5 a.m., when he got up to work. The sweating never awoke him, and caused him no uneasiness. With the exceptions stated his health had been good. Syphilis and alcoholic excess were denied. There was no evidence of phthisis in the family history.

His present attack began last spring. Whilst at work he was troubled with attacks of sneezing. He lost his appetite and became weaker. A month after the commencement of these attacks he was seized one evening with "shivers" while at a friend's house. He went home and had four shivering attacks during the night with pains in the belly, back, and head. His

temperature was 103° F. For a week he was "unconscious," after which he remembers a period of vomiting and constipation. Altogether he remained in bed for seven weeks and started work 10 days later. He had been at work for two weeks when he was seized with severe pains in back and chest and obstinate vomiting, independent of food, and at irregular times. He had also suffered at intervals of a month from severe attacks of shooting pain in the left hypochondrium with localised tenderness over the eleventh rib, which was relieved by pressing directly on the costal cartilage. Drs. Appleton and Whiteley, who had followed the case and sent the patient to the hospital on account of the persistence of the vomiting, accompanied with paroxysmal pain and increasing weakness, had been struck with the obscure nature of some of the symptoms, and after considering the possibility of Addison's disease, they were inclined to attribute the patient's wasting and other symptoms to some form of malignant disease.

On admission into St. George's Hospital, on October 18th, 1899, his condition was one of great debility and almost of prostration. Attention was arrested by the brownish discolouration of the skin without any corresponding staining of the buccal mucous membrane. There was slight pyrexia, some dulness, and a few râles at the right apex. The liver was moderately enlarged, and the spleen also increased in size. One of the left cervical glands was enlarged, but subsequently the swelling rapidly subsided. The general pigmentation of the skin, which seemed greater than that which might naturally belong to an Italian, with dark areas at spots where the clothes or the truss exercised friction and around one or two scars, taken together with the feebleness of the pulse, the great muscular weakness, and the history of progressive emaciation, suggested the possibility of Addison's disease. The peculiar pigmentation of the nails seemed to favour the same view. Though they are usually free from pigmentation a few cases have been observed in which pigment was deposited in them. A further peculiarity, however, was noted, viz., that the brownish discolouration was limited to the distal portion of the nail. That part which was near the lunula was absolutely free from pigment. Further inquiry having elicited the history which is given above, the probabilities of the case seemed to be in favour of the pigmentation being accidental and due to continued exposure to the smoke of heated asphalt. The toe-nails presented similar

appearances to those described above, but this circumstance did not seem to exclude the explanation stated.

The other symptoms and the enlargement of the spleen and of the liver remained to be explained. The association of a glandular swelling and of a marked splenic enlargement with anæmia suggested the view that the case might be one of Hodgkin's disease. This, however, was not supported by any further evidence. The examination of the blood, which was kindly made by Dr. Buckmaster, showed considerable decrease in the number of red cells, with a marked increase in the number of polynuclear leucocytes together with some myelocytes, and a marked increase also in the number of eosinophilic cells. These appearances were consistent with the view that the case might be one of early leucocythæmia.

The patient made good progress during his stay in the hospital under treatment by suprarenal substance, which was prescribed on the strength of the original diagnosis, and which he continued to take in association with iron and quinine.

Dr. Auld did not think the condition could be produced by asphalt working. The pigmentation did not strike him as amounting to much, but the condition of the spleen was certainly remarkable. Had it been Addison's disease, the pigmentation would, by this time, have been much more marked. He thought it was much more likely to be malaria.

Dr. Savill, in reference to the pigmentation produced by malaria, referred to the case of a man he had seen come on board ship, in whom the pigmentation round the eyes made them look as if artificially blackened in a pugilistic encounter, but it was afterwards found that the same pigmentation occurred in other parts of the body. The pigmentation of malaria, such as occurred among inhabitants of the Mediterranean shores, might be very marked and very extensive.

FOUR CASES.

By Harold L. Barnard, M.S., F.R.C.S.

(1) and (2) *Two Cases of Serpiginous Ulceration of the Eyelids.*

The patients were two women; in one there was a history of syphilis but not in the other. The second case began as a little pimple on the side of the nose, which had spread steadily, and had caused ectropion of both lids. There had been considerable prolapse of the conjunctiva of the upper lid which Mr. Barnard

had removed when operating upon the eyelids. He thought the case was probably syphilitic, like the other, but the point of origin and its steady progress for four years suggested the possibility of a superficial rodent ulcer.*

(3) *Nasal Dermoid Cyst.*

The patient, a boy aged 4 years, had had a cyst on the bridge of his nose since birth. There was a dimple at the tip through which a probe could be passed for a short distance. The cyst had been knocked and had been opened several times owing to suppuration. Mr. Barnard took it to be a dermoid cyst, and removed it. He insisted on the extreme rarity of dermoids on the bridge of the nose, adding that he could not explain its presence in that situation unless it were an inclusion of skin occurring during the fusion of the lateral portions of the fronto-nasal process.†

(4) *Hernia Cerebri due to Tumour.*

Mr. Barnard also showed a man, aged 20 years, who had come to him with symptoms of cerebral tumour of four years' standing. The patient had double optic neuritis, followed, later on, by hemianopia on the left side. Eight months ago there was continuous vomiting, with severe and persistent headache, and the sight was practically lost. Dr. Cautley had suggested that an attempt should be made to relieve the pressure, and Mr. Barnard removed a large piece of the skull. An enormous hernia cerebri formed beneath the scalp, but the result had been good in respect to the symptoms. After the operation the patient recovered sight sufficiently to get about by himself, and the headache and vomiting had subsided. He had been in his present condition for eight months. Mr. Barnard commented on the long duration of the symptoms of cerebral tumour—four years—and on the extreme thinness of the skull, which was hardly thicker than parchment, and had perforated at one spot on the frontal bone.‡

* It proved to be rodent, and both eyelids with the surrounding skin were removed and replaced by a flap from the forehead.

† The cyst recurred, and a probe could then be passed into it from the dimple. The whole sinus was then excised and the case finally cured.

‡ This case was an endothelioma of the centrum ovale of the posterior frontal lobe.

TWO CASES.

By F. C. Wallis, F.R.C.S.

(1) *A Simple Comminuted Fracture of the Tibia; and* (2) *A Pott's Fracture* (*both treated by wiring*).

(1) The patient, a man aged 23 years, was admitted into the Metropolitan Hospital on January 10th, 1898, suffering from a fractured left tibia and fibula. The accident was caused by the explosion of a toy boiler which hurled the man against a wall, his leg also being struck by some of the fragments. The tibia was fractured near the middle, the fibula somewhat lower. There was great displacement of the tibia and comminution of the fractured ends. After 10 days in splints, as the leg was much swollen, and the fractured ends were in extremely bad position, the man was anæsthetised and a long incision over the seat of fracture was made. A great deal of blood clot was washed away and various pieces of loose bone were removed, one piece being of considerable size. The muscles were lacerated, and a portion of one had become wedged in between the bones. It was necessary to saw off three-quarters of an inch of the upper fragment before the two ends could be brought together. The recovery was straightforward and uneventful, and the necessary shortening of the tibia had been overcome by the patient wearing a somewhat thicker sole on that side.

(2) The patient, a man aged 63 years, was admitted to the Metropolitan Hospital on May 27th, 1899, suffering from a left Pott's fracture, caused by his slipping off a doorstep and falling with his left ankle under him. On admission the left foot was much everted and displaced backwards. The inner condyle of the tibia was found to be broken, and the upper fragment protruded close under the skin; there was also considerable swelling around the joint. The fibula was fractured in the usual situation. The patient was gouty and bronchitic, and had a slight cloud of albumin in his urine. It was most undesirable to keep him on his back longer than necessary, and all endeavours to get the foot into good position were futile. On June 2nd the patient was anæsthetised, and a curved incision $4\frac{1}{2}$ inches in length was made behind the internal malleolus, and a flap of skin turned

right forwards. After washing away the blood clot the internal malleolus was seen to have retreated almost within the ankle joint and to be covered in with torn aponeurosis. After the fractured ends had been cleared of all clot a wire was passed through them, and, when this was fastened up, the foot came into most excellent position, the fibula not being interfered with.

The after history, as far as the wound was concerned, was uneventful; but a sharp attack of gout and some general bronchitis made the patient's convalescence somewhat tardy. The patient walked out of the hospital on July 14th with good position, good movement, and no lameness.

ŒDEMA OF ARM AND FOREARM FOLLOWING AXILLARY ABSCESS.

By John H. Morgan, F.R.C.S.

Mr. Morgan showed a young man who had had an abscess in the axilla three years before. The cause was unknown and the patient had undergone extensive surgical treatment at a hospital in Carlsbad, with the result that he had a periodical condition of solid œdema affecting the arm and forearm, but, curiously enough, not involving the hand. Mr. Morgan presumed that it was originally a case of lymphatic abscess with subsequent blocking of the lymphatics of the limb.

February 12*th*, 1900.

CASE OF RESECTION OF THE SMALL INTESTINES IN A BOY.

By F. C. Wallis, F.R.C.S.

The patient, aged 7 years, about two years previously had been under treatment for tuberculous peritonitis in a London hospital, and about a year after that acute intestinal obstruction had occurred. For this the abdomen was opened, and a piece of small intestine above the obstruction was sewn to the abdominal walls, opened, and a fæcula fistula resulted. The patient then

went to St. Monica's Home, where the operation of enterectomy was performed by Mr. Wallis on December 1st, 1899. Five inches of small intestine were removed, and the cut ends were joined by interrupted sutures. The boy made a very satisfactory recovery, in spite of the septic conditions of the skin, which, for a considerable area round the original wound, was red, inflamed, and discharging pus, on account of the irritation and partial digestion caused by the contents of the small intestine constantly flowing over it. The pieces of resected intestine were shown.

Mr. G. R. Turner (the Vice-President in the chair) asked whether, at the previous operation, there had been any inflammation round the stitches or elsewhere to cause the formation of adhesions and shut off the general peritoneal cavity.

Mr. Wallis, in reply, said he worked with a certain feeling of security because he was dealing with a shut-off portion of peritoneum. There was no suppuration of the stitch-holes when the stitches were removed on the eighth day; but at the edge, where the intestine had originally been sutured to the skin, there was a little coloured serous exudation. Since then he had operated in two other cases, sewing together the large intestine and adopting the plan of putting sutures through every structure except the skin, the edges of which he left a short distance apart. This had the advantage of doing away with the danger of subcutaneous collections, which often gave rise to trouble.

CASE OF URTICARIA PIGMENTOSA.

By Francis Jaffrey, F.R.C.S.

The patient was a boy, 14 months old. The condition first appeared when he was 4 months old, "following vaccination" (?). He was brought to the out-patient department of the Belgrave Hospital to have a large nævus of the right side of the thorax treated, and on examination he was found to have spots of pigmentation all over the body. They were most marked on the lower part of the abdomen and on the back, also on the arms, legs, face, and even scalp. The mother stated that the wheals appear singly, but occasionally several have appeared in different parts of the body at the same time. They varied in shape, and in size from a small pea to a shilling-piece or even larger. They did not appear to itch, as the child did not scratch them. They lasted for a day and then disappeared, leaving a brown stain, which was darker at the margin than at the centre, but was not

raised. The child had an attack of measles on Christmas Day, and the wheals had not reappeared until February 11th. He was a bottle-fed child, the youngest of five, and the others were quite healthy. In looking over the various text-books on diseases of the skin, Mr. Jaffrey found about 50 recorded cases, the majority of which were in boys, and were stated to have commenced a few months after birth. As to the diagnosis, he admitted that he was in some doubt, as the case did not exactly correspond with the descriptions given, the pigmentation being only slightly raised. As to treatment, he was only giving mild purgatives.

Dr. EWART asked whether any arsenic had been given, whether there was any enlargement of the spleen, whether the blood had been examined, and whether there was any special neurotic symptom or other constitutional peculiarity in the child or in the family.

Mr. JAFFREY, in reply, said he had discovered no enlargement of the spleen, and, so far as he was aware, no arsenic had been given. He had not examined the blood, but would place specimens at Dr. Ewart's disposal for that purpose if he wished.

CYANOSIS FROM BIRTH—ABSENCE OF CARDIAC MURMUR.

By CHARLES W. CHAPMAN, M.D.

F. G., a girl aged 18 years, was admitted under Dr. Chapman's care at the National Hospital for Diseases of the Heart on November 21st, 1899. She complained of constant dyspnœa, irrespective of exertion but much aggravated by it; præcordial pain, which at times shot down the left arm to the finger-tips, and was followed by temporary numbness. At other times the pain commenced at the fingers, and shot upwards to the shoulders. Excitement or exertion was sometimes followed by unconsciousness for ten minutes; during these attacks she had at times bitten her tongue. There were also occipital pain and vertigo.

History.—She had been always short of breath, and had blueness of the skin. At 12 years the patient became much worse, and had a sharp attack of hæmoptysis, since which time her condition had been much as it was now. The family history was good, with the exception of epilepsy in one brother, who was now in an asylum.

On admission the patient was well nourished, and her face presented a somewhat bloated appearance. There was general cyanosis and a little clubbing of the fingers and toes. The cardiac dulness began just above the third rib, laterally to the left, to half an inch outside the nipple; there was but little increase of dulness to the right. The apex-beat was at the lower part of the fourth space, $2\frac{1}{2}$ centimetres to the right of the nipple line. There was pulsation to be felt in the left parasternal line, at the level of the third intercostal space. There was no thrill to be felt, or murmur to be heard. There was good breathing in the lungs, and the liver was not enlarged.

The cyanosis in this case must be due to deficient aëration of the blood, since the condition of the lungs and liver negatived the possibility of its being caused by backward pressure. It was difficult to say what was the condition of the heart in this case, but from reference to a case reported by Dr. Pye-Smith in vol. xxii of the 'Transactions of the Pathological Society,' and to a similar one reported to the same Society in 1880, it would appear probable that the case before them was one of transposition of the trunk vessels at the base of the heart, so that the aorta sprang from the right ventricle, and the pulmonary artery from the left ventricle. It was also probable that there was free communication between the auricles, and that the ductus arteriosus was patent.

Dr. Chapman mentioned that Dr. Lees had examined the patient, and he was of opinion that the cardiac condition was as stated. At the suggestion of Mr. Adams Frost the eyes were examined ophthalmoscopically, when both arteries and veins were found to be much dilated.

Dr. Ewart remarked that, whilst auscultation revealed no evidence of abnormality, percussion detected a dulness running up from the heart to the inner end of the left clavicle, which might be due to a considerably dilated ductus arteriosus. A possible explanation of the absence of auscultatory signs might be the size of the abnormal orifice, which might be such as not to give rise to murmurs. He preferred that view to the explanation suggested by the author, against which was the fact that the aortic and pulmonary second sounds appeared to be normal, and were heard in their respective regions.

Sir Hugh Beevor commented on the very marked pulsation in the second left interspace, although the dulness was very much contracted at that point. It was such as one met with in cases of dilated right ventricle stretching up to the pulmonary artery; but in this case there was no increase in the size of the right side of the heart, and this, he thought, confirmed the idea put forward by the author.

CASE OF CARDIAC DISEASE WITH PULSUS BISFERIENS.

By Sir Hugh R. Beevor, Bart., M.D., F.R.C.P.

The patient was a man, aged 23 years, who had been subject to dyspnœa on exertion since an attack of chorea at the age of 11 years. Cardiac and arterial symptoms of aortic regurgitation and stenosis were present. Pulse tracings which were shown adduced confirmatory evidence. The symptoms pointed to the existence of a contracted aortic orifice, but not to any great degree of narrowing. The effects of mitral incompetence had been apparent from time to time during the last two years, for the patient had had œdema and hæmoptysis, and the liver now extended just beyond the margin of the ribs.

CASE OF CONGENITAL DISLOCATION OF THE HIP.

By Arthur E. J. Barker, F.R.C.S.

The patient, a girl aged 4 years, presented the usual waddling gait with marked lordosis. As was often the case, nothing abnormal was noticed until the child began to walk. Mr. Barker showed skiagrams in which the head of the bone was seen to be rotated forwards, instead of backwards, as was usually the case in traumatic dislocation. He doubted the value of operative treatment for the purpose of remedying this deformity. He mentioned that Lorenz was stated to have operated by bloodless or by cutting methods in 360 cases, Hoffa in 170, and Mickulicz in 100. Lorenz had met with a number of misadventures as the result of his manipulations, including one case of gangrene of the thigh in a girl of 13; 10 fractures of the neck in patients between 9 and 16; one fracture of the horizontal ramus of the pubis; one infraction of iliac bone; two cases of paralysis of the perineal nerve; five cases of paralysis of the quadriceps (recovered from); and three deaths under chloroform. He commented on the remarkable number of cases dealt with by Lorenz, who had had 135 skiagrams made of earlier and later cases. In 24 cases

of supposed reduction, the head had no relation of contact with the acetabulum, but lay against the anterior inferior spinous process, or even higher. In 27 cases it lay upon the upper border of the socket; in 32 it was excentric to the socket, but the latter had thrown out a brim of bone around it; in 47 the head seemed to be really replaced over the socket. Mr. Barker observed that it was often comparatively easy to bring the head nearly down to the level of the acetabulum, but it could not be kept there, and, in view of the risks it entailed, he did not think that the method of forcible reduction would commend itself to surgeons in this country.

Mr. Wallis said he had seen, at the Clinical Society, a good many of these cases after operation. What had always struck him about the supposed benefit was that it was extremely limited. That was particularly brought out on one occasion when two patients were shown, one of whom had been operated upon, and the other not: the latter walked much better than any patient he had seen after a more or less formidable operation.

Mr. Lynn Thomas said he had operated upon four cases. One was a double congenital dislocation, in which he had divided the tendons and reduced the bones; the patient was then kept on a double Thomas's splint for nine months, after which he was allowed to walk. At the present time both the patient and his parents were very pleased with the result. He had had a skiagraph taken recently, which showed that the head of the bone, instead of being in the acetabulum, was on the dorsum, if anything rather higher up than before, yet the waddling gait had disappeared.

THE TREATMENT OF PULMONARY EMPHYSEMA BY CONTINUOUS ELASTIC PRESSURE.

By Wm. Ewart, M.D. Cantab., F.R.C.P.

Leaving aside consideration of its general and of its medicinal treatment, emphysema, a disease in great part due to mechanical causes, needs mechanical remedies. However much the latter may vary, their object is the same, to reduce to an average size the enlarged thorax and the distended lung, and to restore the fulness of their respiratory movements. To that end the pressure of the atmosphere has been utilised, either in connection with the external thoracic surface as in the compressed air bath, or with the internal pulmonary surface as in the method of expiration into rarefied air. Various mechanical pressures have also been

devised for the thorax, in addition to simple respiratory exercises. But with the exception of a residence at selected levels, none of these forms of treatment are capable of continuous application; some of them are not quite easy to apply, and none are suitable to all stages.

The treatment by continuous elastic pressure seeks to fulfil these indications, and, if possible, to check the disease by forces as slight, as gradual, and as continuous as those by which its destructive results are progressively evolved.

The "automatic respiratory jacket" is intended to be worn night and day. It is provided at the sides with an elastic part, the tension of which can be adjusted to suit varying requirements. A description of the jacket will be published jointly by the author and by Dr. H. B. Roderick, whose help, both in the planning and in the construction of the jacket, was most valuable. A yet simpler home-made modification of the same appliance is exhibited under the sufficiently descriptive name of the "respiratory waistcoat." In both instances the tension is to be adjusted by buckles so that each inspiration meets with a slight resistance, and each expiration with a slight pressure of recoil. The chief theoretical indications of the mechanical treatment of emphysema are thus fulfilled. In the present case, that of a boy aged 12, with considerable emphysematous deformity and respiratory impairment, as well as in other cases now under treatment, considerable benefit has resulted. The boy, who has worn the jacket for six weeks, still finds comfort in the treatment; his chest measurement at the nipple level has decreased, and there is considerable gain in the respiratory excursions (the range has increased from three-quarters of an inch previously to one and a-half inches now). His breathlessness on exertion is also much less, and his muscular strength decidedly better. There is good reason to believe that considerable further improvement may be noticeable when the patient is exhibited again at some future date. It is needless to add that the treatment is most likely to be of use in early life and at an early stage of the affection.

APPARATUS FOR OVERCOMING THE DISTENSION OF THE AIR CELLS IN EMPHYSEMA.

By E. H. Willock, M.R.C.S. Eng., L.R.C.P. Lond.

Mr. Willock showed an apparatus which he had devised for the purpose of overcoming the distension of the air cells in emphysema. The apparatus comprised a series of pulleys running into a kind of corset, and so arranged that when the patient lifted the arms with inspiration the pressure was relaxed, and when the arms were brought down with expiration considerable pressure was exerted on the chest. The methodical use of this apparatus gave much relief. He had used it in one case of phthisis to promote respiratory gymnastics, and the patient had greatly improved under the treatment.

Dr. Ewart thought that Mr. Willock's apparatus was well adapted to carry out the ingenious purpose in view. He suggested that its efficiency might be further increased if the pulleys could be made to take action vertically over the shoulders as well as horizontally around the chest, so as to compass the thorax on all sides.

CASE OF TUMOUR WITH DEFORMITY OF THE FEMUR (OSTEITIS DEFORMANS).

By W. H. Battle, F.R.C.S.

The patient, a man aged 29, first complained of pain in the left femur about two and a-half years ago. The attacks of pain came on at irregular intervals, but had ceased for some months. There had been a gradual bending backwards of the bone, formation of swelling at the point of bending, and shortening of the limb, which measured 2 inches more in circumference at the point of greatest thickness, and it was 2 inches shorter than the other. No other bones were affected. About a month before the man suffered from headache, but this had disappeared under treatment by iodide of potassium. There was a well-marked history of syphilis seven years before. Under the X rays it was possible to see a general bony increase of the shaft of the whole femur, the deposit being most marked at the point of greatest bending. Nowhere was there any sign of remaining compact

tissue, which appeared to have been replaced by cancellous tissue. There was a curious deformity of the left femur, which bent backwards about 6 inches from its lower end, and at this point it presented a well-marked bony tumour. There was general enlargement of the whole of the bone, especially at the point of bending. In front the muscles were continued over the convexity in a state of hollow tension. The man could walk about and complained of no pain. Mr. Battle was inclined to regard the case as one of osteitis deformans affecting only one bone and accompanied by a posterior curvature instead of the usual one forwards and backwards. Such cases were very rare, and he had only been able to find one like it described, and that was by Mr. Bowlby in the 'Pathological Society's Transactions, 1883.'

TWO RARE CASES OF RHEUMATISM.

By W. J. Hadley, M.D. Durh., F.R.C.P. Lond.

The first was a case of osteo-arthritis in a female child aged 8 years. During the previous three years she had had five attacks of rheumatism. The first three were ordinary polyarticular rheumatism, which cleared up completely; but the fourth and fifth had left some deformity in the joints. The present attack started in April, 1899, and she was now completely crippled. There was considerable deformity and enlargement of most of the joints, which skiagrams showed to be due, in the larger joints, to increase of the soft tissues; no alteration in the bones was visible. The right hip was fixed and the neck was stiff. There was a systolic bruit at the base which was tending to become less marked, and was probably of hæmic origin. The spleen was slightly enlarged. Anti-rheumatic medicines had proved useless. Massage and daily sweating of the joints had given fairly satisfactory results. There had been occasional slight variations of temperature. The second case was that of a girl, aged 9 years, who had had chorea three years before. There was no history of rheumatism or of joint affection at any time. The present illness began, a week before admission to hospital, with pains in the limbs, headache, and, two days before admission, severe pain in the right foot. There was no definite arthritis

at any time. On admission there was well-marked mitral regurgitation with cardiac hypertrophy; the spleen was enlarged, firm, but not tender. After three days of irregular pyrexia there was a sudden rise, accompanied by dizziness, faintness, and vomiting (probably of embolic origin). Ten days later a similar attack, though without pyrexia, was attended by pain in the lumbar region. A presystolic mitral bruit and a systolic aortic murmur also appeared. Later the occurrence of sudden pain in the left arm, with tenderness and other symptoms, pointed to embolism of the brachial artery, and the radial artery had since been pulseless. The first case was interesting by reason of its rarity. It was a typical case of rheumatic arthritis occurring in a child, and tallied with the cases reported by Dr. Still. The interest in the second case lay in the occurrence of the emboli which had arisen in the course of endocarditis (certainly of rheumatic origin) in a child.

A NEW TOURNIQUET.

By J. Lynn Thomas, F.R.C.S.

Mr. Lynn Thomas, of Cardiff, showed a forceps which he had devised to serve as a tourniquet in amputations through the thigh and at the hip joint. It had two blades, $4\frac{1}{2}$ inches long, of which the under one was smooth and probe-pointed. This was thrust in under the main blood-vessels and gave absolute control over them. He had used it in other operations—amputation of the arm, the shoulder joint, the penis, and, though he had kept it on for 50 minutes at a time, it had never given rise to any vaso-motor paralysis. The forceps-tourniquet was easily kept surgically clean and was under the control of the operator.

TWO CASES ILLUSTRATING VARIATIONS IN INGUINAL HERNIÆ DUE TO ABNORMAL DESCENT OF THE TESTIS.

By W. McAdam Eccles, M.S., F.R.C.S.

Case 1 was that of a man, aged 23, who presented imperfect descent of both testes, and on the right side he had a large interstitial hernia when he was first seen some eight months ago.

He refused to have an operation performed, preferring to be treated by the application of a truss. He now had but little evidence of his former hernia, which had mounted up to within a short distance of the anterior superior iliac spine. It would be observed that his right testis lay in the inguinal canal close above Poupart's ligament, and that his left testis was just protruding at the site of the deep (internal) abdominal ring. Neither of his superficial (external) rings can be easily made out on account of their small size.

Most interstitial herniæ were readily reducible and could be retained, if operation were contra-indicated, by a truss, generally by one bearing an interstitial plate.

As to the causation of this form of hernia, it was perhaps brought about by the partially descended testis barring the exit from the canal and inducing the sac to expand in the plane of least resistance. But that this was not the sole factor responsible for an interstitial hernia was evidenced by these herniæ occurring in females. Here, and likewise in many male cases, the superficial (external) ring was practically non-existent.

Case 2.—This man was another instance of the variation in the form of an inguinal hernia by association with an abnormal descent of the testis. The patient was aged 42, and for the last 20 years at least he had been conscious of a right inguinal hernia. He was not very clear as to the position of his right testis prior to the development of the hernia, but he gave the history of "a painful lump in the groin" ever since he could remember. On examination he presented a well-marked cruro-scrotal hernia, that is to say, an inguinal hernia, which, escaping from the superficial abdominal ring, had missed the entrance to the scrotum, and had stretched the skin forming the junction of the thigh and the scrotum. It would be noticed that the scrotum itself was developed, though it was undistended by the presence of a testis in its right half. The most external covering of the hernia was, therefore, ordinary skin, and there was no dartos tissue in it. It would, moreover, be observed that the deep layer of the superficial fascia had apparently given way at one point, owing probably to the fact that a hydrocele was tapped 20 years ago, and had allowed the peritoneum forming the sac-wall to protrude as a loculus, the edges of the aperture being sharp and readily felt. If the contents of the sac were reduced, the right testis would be found lying in the right half of the perineum close to the ramus of the ischium,

and not far from the level of the anterior end of the anal margin. It was small in size, and very tender to the touch.

The probable clinical history of a hernia of this form was that there had been an ill-developed small testis, which had descended along the canal and had extruded at the superficial ring. Then a hernia had been formed in the patent process associated with this testis. Subsequently the testis had been partly drawn by the perineal band of the gubernaculum, and partly pushed by the hernia into the perineum, having for some reason missed the proper way into the scrotum. The hernial protrusion following the testis had itself refrained from passing into the perineum, but had stretched the cruro-scrotal fold. Such cases were eminently suitable for operation, with a view of curing the hernia and placing the testis in its own half of the scrotum.

Mr. Battle, in reference to the case described as cruro-scrotal hernia, asked if they were expected to look upon this as a new variety of hernia. He thought it was an inguinal hernia modified by a secondary traumatism.

Mr. Barker said he had recently operated on a somewhat similar case occurring in a woman. The case showed that the position of the testicle had nothing to do with the production of interstitial hernia, and, moreover, that one could have both forms on the same side. In this woman one sac ran up to the anterior spine, and the other down into the labium, both having a common neck.

Mr. G. R. Turner said that when he was operating upon a case of undescended testis with hernia the other day, he was surprised to find two distinct sacs—one connected with the testis, an ordinary congenital sac, and the other, quite distinct, running right up behind the cord. In answer to Mr. Barker, he said that he did not come across the epigastric vessels.

Mr. McAdam Eccles, in reply to the remarks of Mr. Battle, said that he still adhered to his belief that the second case was one of true cruro-scrotal hernia, but he was willing to admit that this was no newly-described form of protrusion. He called attention to the fact that the right half of the scrotum was developed though undistended, and that the sac was wholly covered by ordinary skin, not containing any dartos tissue. The lateral bulging from the sac was entirely secondary, and no doubt due to the traumatism. Mr. Barker had alluded to the fact of a sac being found associated with an interstitial hernia, lying in the scrotum or in the labium. Mr. Eccles thought that this was always secondary to the interstitial portion. In the male subject, if the primary part of the sac was due to a partially-descended testis, then this organ was usually pushed down into the scrotum when the secondary part of the sac was developed. Very severe strangulation is liable to occur in these secondary portions, and *reduction en masse* may result from taxis, the knuckle of intestine being pushed back into the interstitial portion. The President's case he would regard as probably a form of "infantile" hernia—that is, an acquired sac behind a patent processus vaginalis.

CASE OF DEXTRO-CARDIA.

By Sarat K. Mullick, M.B., C.M., Edin.

The patient, a labourer aged 22, had come to the out-patient department of the Heart Hospital, Soho Square, complaining of pain, palpitation, dyspnœa, vertigo, faintness, and flatulence. The pain was sometimes sharp and piercing, at other times dull and heavy. It was confined to the right mammary region, and often became worse two hours after a meal. His previous history was satisfactory, and no hereditary taint could be traced. Until about five months before the man had enjoyed good health. One day in August, when ascending a ladder after dinner, he was suddenly seized with palpitation and, descending with difficulty, he was obliged to cease work and take to bed. After a few days he complained of pain over the right mammary region, followed in a day or two by dyspnœa. Vertigo and faintness supervened about two months afterwards.

The apex beat was on the right side of the chest in the sixth interspace, and the cardiac impulse could be seen in the fourth and fifth interspaces. The præcordial dulness extended $11\frac{1}{2}$ centimetres to the right of the mid-sternal line. There was no cardiac murmur. The liver and stomach were also transposed to the left and right sides respectively.

Dr. Garson pointed out that the area of the aorta appeared to be much lower than normal. The case recalled to him a similar one of transposition of viscera, preserved in the Museum of the Royal College of Surgeons.

April 9th, 1900.

CASE OF MYOTONIA CONGENITA (THOMSEN'S DISEASE).

By Leonard G. Guthrie, M.D. Oxon.

The patient was a boy, aged $10\frac{1}{2}$ years, who had been brought to the hospital on account of the awkwardness of his movements. The hands exhibited a peculiar condition of tonic spasm somewhat resembling tetany. The wrists also were flexed towards

the ulnar side, and the elbows were semi-flexed. When asked to grasp the observer's hand he did so with apparent effort and could not let go easily. Sometimes he could not take hold of an object without first pulling his adducted thumb outwards with the fingers of his other hand. For this reason he had a difficulty in feeding himself at times. All the muscles of the upper extremities were in some degree affected. Similar transient spasticity affected the lower extremities. His gait when starting was jerky, and he had a difficulty in getting upstairs. This condition of tonic spasm varied greatly, and was always more marked after rest, and lessened after exercise or when his attention was distracted from himself. The spasm persisted during sleep. He complained that his legs and arms always felt stiff, but he had no pain. The case seemed to be an early stage of Thomsen's disease although the family history of the complaint was wanting.

Dr. Ormerod observed that it was hardly a typical case of Thomsen's disease; many of the features—enlargements of muscles and peculiar electrical reactions being absent. He recalled a case in which there was a similar want of power to relax the grasp, and which was diagnosed as congenital cerebral diplegia (Little's disease). In that case the symptoms did not appear till adult life.

Dr. Savill said the case reminded him of one brought before the Clinical Society some years ago, in which the diagnosis was referred to a committee of inquiry. It was brought forward by Dr. Bolton Tomson, of Luton. The patient was a man, aged 35, in whom the spasm after muscular exertion could also be brought on by exposure to cold. It did not prevent him following his occupation, but was extremely inconvenient. The committee were unable to arrive at any conclusion on the subject, for, although the case resembled Thomsen's disease in some particulars, it did not agree with the recognised description of that malady. He suggested that there were probably many of these spasmodic affections, and it would be very interesting if one could group them.

Dr. R. Maguire said that whatever might be the matter with the patient, he certainly was not suffering from Thomsen's disease. He had never heard of the condition described as Little's disease, but understood what was meant from Dr. Ormerod. One had to decide what was the real pathological condition from which the patient was suffering, and this, he suggested, was the lack of the higher cerebral control which left the spinal centres to their own free will. He pointed out that when left at rest the patient became spasmodic; but as soon as he began to use his cerebral control his muscles worked better. Dr. Maguire did not believe that there was any organic disease, and he thought the boy would improve after puberty.

Dr. Guthrie, in reply, did not wish to speak positively about the diagnosis, nor could he speak definitely about the electrical reactions. The spasm was a little more marked after cold. In support of the suggestion that it was in some way connected with deficient cerebral control, the spasm seemed to be a trifle more marked when the boy's

attention was taken away from the contracting muscles. He would endeavour to show the patient again on some future occasion, by which time, perhaps, the symptoms would be more marked.

CASE OF SKIN PIGMENTATION.

By T. D. Savill, M.D. Lond.

The patient, a woman aged 24 years, presented a curious pigmented skin lesion which went on to vesiculation, and did not belong to any known type of disease. She had come under Dr. Savill's care for severe headache associated with marked visual error. One lung was also the seat of tuberculosis. For eight or nine years the skin lesions in question had appeared in succession on different parts of the limbs and trunk. Each lesion was preceded by a smarting pain, followed by the appearance of an ecchymosis which subsequently swelled up and became an erythematous area varying in size from a bean to a half-crown. One or two days later a vesicle or bulla containing dark brown serum would appear on the top of the swelling. These lesions appeared in succession at irregular intervals of a few days. One or more generally came after a severe attack of headache. She had never more than two or three at one time, but at the time the case was shown she had some 10 or 12 in different parts of the body showing different stages of the eruption. The patient was also the subject of leucodermia, and, in one or two instances, the bullous lesion appeared in the centre of a patch of leucodermia. The condition resembled hæmorrhagic pemphigus in some respects, but it was probably a neurosis, possibly being artificial.

Dr. Eddowes conceived it possible that many of the points had been exposed to pressure, and he suggested the desirability of ascertaining whether the patient was peculiarly susceptible to bruises. He recalled the case of a woman who had come to him at the hospital with bruises all over the body, there being, for instance, a ring round the waist where the petticoats were tied. A week later the woman died suddenly, and he was applied to by the police whose attention had been called to the fact that her body was covered with bruises. He was enabled, however, to show that these were not due to the brutality of her husband, upon whom suspicion had most unjustly fallen.

Dr. Seymour Taylor suggested, in view of the fact that she had tuberculous disease of the lung, that the skin lesions were possibly of tuberculous origin. He referred to some cases with very similar lesions,

shown by Mr. Hutchinson and attributed to tuberculosis. He pointed out that the absence of the tubercle bacillus would not necessarily disprove this view.

The PRESIDENT said he had had a large experience of tuberculous disease, but he had never met with any cutaneous affection similar to those observed in this case. He did not think that they were in any way associated with tubercle or the tubercle bacillus.

Dr. SAVILL, in reply, admitted that the lesions shown suggested the possibility of injury, and that it was impossible to affirm that this was not the case; but there were others in other parts of the body which were out of the way of injury. He agreed that it might be worth while making a bacteriological examination of the serum, although the case did not strike him as being probably one of tuberculous disease.

CASE OF "LATE RICKETS."

By E. MUIRHEAD LITTLE, F.R.C.S.

THE patient, a girl aged 17 years, was well until she was 5 years of age, when knock-knee appeared. At seven she had to take to crutches. She was said to have broken both thigh-bones, the one six and the other five years ago. When first seen, in October, 1899, her length in the recumbent position was 44 inches. The head and ribs showed signs of rickets. The lower end of the radii and ulnæ were much enlarged. There was cross-legged progression. The shafts of the femora were very much curved forward and outwards; the tibiæ were normally straight. Macewen's operation had been performed on one knee, and the bone, instead of being very hard, had proved to be so soft that a mallet was scarcely necessary. Skiagrams showed the bones to be unusually pervious to X rays as compared with normal bones. The quantity of calcium present in the urine was less than half the normal, otherwise the urine was normal. Similar cases recorded by Dr. Drewitt and Mr. Clutton were referred to. The marked enlargement in these cases distinguished them from so-called rachitis adolescentium.

Mr. KELLOCK observed that although it did not much matter what a disease was called, provided its nature was understood, yet he thought it a pity that the same name should be applied to two diseases which appeared to be so different. Although this case might be one of what was called "late rickets," it looked more like a disease affecting the whole length of the bones rather than one affecting the epyphysial ends, and he thought it one of mollities or fragilitas ossium. The extreme softness of the bones, found at the time of the operation and shown to have existed

before then by the occurrence of fractures of both femora, was not consistent with rickets, especially with rickets of long standing, for then one would have expected the shaft of the bone to be as firm or firmer than normal. The use of crutches by the patient rather suggested that she had sought relief from pain from pressure on standing. He referred to a very similar case which had been under his care at a convalescent home, where the deformities in the bones had been very much like those in this case. A cuneiform osteotomy had been performed on the tibia with the result that no bony union had occurred, and, whilst under observation, fractures of both femora occurred from slight causes. In that case it had been ascertained that the child's mother and aunts had suffered from the same condition.

The PRESIDENT said it was difficult to define exactly what was meant by several terms applied to these cases in which the bones were affected. He thought this one was really a case of "late rickets." There was distinct enlargement of the ends of the bones, and the skull was decidedly ricketty. The fact that the changes in the chest were not such as one got in the very early cases, was in favour of "late rickets." He contrasted the case with one he had had under his care at University College Hospital, in which a girl of 15 exhibited the effects of rickets from early childhood, in an intense degree, and presented the appearance of a child of six. In this case the thorax was greatly deformed.

Mr. MUIRHEAD LITTLE, in reply, challenged the assertion that rickets only affected the epiphyses, the generally received view being that the disease affected the whole growing bone. Fracture of bones, too, was, he thought, common in rickets, usually green-stick fractures such as this patient's probably were. He pointed out that the fractures occurred three or four, and not eight or nine years after the onset of the disease. The patient had never complained of any pain, and used crutches simply because her knees gave way. He pointed out also that one did not get enlargement of the epiphyses in mollities or fragilitas ossium ; moreover, in the latter the urine would contain an excess of salts which was not found in this case. He thought the bones were beginning to harden, and he might be able to show her again in a few years. He mentioned that the patient came from Stroud in Kent, which was a very chalky district, so that there had probably been no deficiency of lime in her diet.

CASE OF ABDOMINAL ANEURYSM.

By H. A. CALEY, M.D., F.R.C.P.

THE patient, a plasterer, aged 37, came to the surgical out-patient department of St. Mary's Hospital on account of a varicose condition of the veins of the left leg and thigh. Three years before he had been laid up with swollen veins in the left leg, which subsided after six weeks' rest in bed. From that time no further trouble had been experienced until January of this year, when the veins of the left leg and thigh were noticed to be distended, and the

former at times swollen. There was a history of syphilis 16 years ago, but none of injury or strain. Excepting for occasional slight discomfort after meals, apparently dyspeptic, there was no pain in the abdomen or back; during the last few months, however, he had at times been conscious of a "beating" sensation in the abdomen. On examination, the veins of the left leg and thigh were seen to be much distended, but there was no œdema. In the umbilical region there was a large pulsating tumour extending to the right along the course of the common iliac artery. The right margin of the tumour was well defined and hard; there was marked expansile pulsation, and, on pressure, a slight systolic thrill and a faint systolic bruit. There was no perceptible pulse in the right external iliac or femoral arteries, but an exaggerated pulsation along the left external iliac as far as Poupert's ligament, and a strong femoral pulse on that side. The urine contained albumen in fairly constant amount, usually about 4 grammes per litre, urea, 1·75–2 per cent., occasional hyaline casts; some arterio-sclerosis, heart not enlarged, and otherwise nothing noteworthy.

Remarks.—The physical signs of aneurysm were unequivocal, and it was evident that the sac involved the abdominal aorta near its bifurcation, the right common iliac, and almost certainly the left common iliac also, whilst the abnormal pulsation in the left iliac region suggested some associated dilatation of the left external iliac. With absolute rest in bed, restricted diet, and large doses of potassium iodide the pulsation had much diminished in extent and in degree, the tumour had become more solid, and the distension of the veins of the left lower limb had almost disappeared. The chief points of interest were the absence of pain, the considerable vertical extent of the arterial lesion, and the degree of improvement under strict treatment. The absence of pain was probably chiefly attributable to the fact that the sac had originated in the lower part of the abdominal aorta, near the bifurcation.

TWO CASES.

By Alfred Eddowes, M.D. Edin.

(1) *Psoriasis and Osteo-arthritis.*

These occurred in the same patient, a married woman, aged 31. She had been married 10 years, without children, and there was no history of rheumatism or gout in the family. The outbreak of psoriasis commenced 17 years ago, simultaneously with the appearance of menstruation, a coincidence not uncommon. Five years ago she suffered a good deal of worry and mental strain, and four years ago the enlargement of the joints had commenced.

Dr. Eddowes showed photographs illustrating a similar combination of disease in a male patient, aged 33 years. He believed there was an undoubted connection between the rheumatic diathesis and psoriasis.

(2) *Simple General Icthyosis.*

The patient shown was a youth suffering from this condition, on which Dr. Eddowes intended to try the effect of thyroid gland.

CASE OF DIVER'S PARALYSIS.

By Robert Maguire, M.D., F.R.C.P.

The patient, a man aged 45 years, had been a deep-sea diver, and had worked at almost the greatest possible depths. Two months ago he was working on a wreck on the coast of Florida, and descended to between 28 and 29 fathoms. He had his air-tube and life-line fouled, and when these were released by the man on watch, "he," as he says, "popped" and came up 4 feet above the water line. Since then he had suffered from paraplegia, the left leg being the worse. Dr. Maguire thought that the whole condition was due to sudden diminution of pressure on the peripheric vessels.

Dr. Gossage mentioned that this same patient had been under his care, and then he had had loss of sensation. This was, altogether, the seventh attack of paraplegia. The former attacks had been associated with a good deal of alcohol.

Dr. Calvert mentioned that there had been two cases of diver's paralysis during the last five years at St. Bartholomew's Hospital, and one of them came from Florida. He asked whether there had been bleeding from the nose and ears.

Dr. Maguire replied in the affirmative.

CASE OF REPEATED FRACTURE OF THE PATELLA.

By Frederick C. Wallis, F.R.C.S.

The patient, a man aged 69 years, first fractured his left patella 35 years ago. Since this fracture, which was treated by rest, the patient had refractured the patella twice, the last time about five months ago. He was a carpenter by trade and had, until the last few weeks, been quite able to do his work. His present condition showed that the main part of the patella was widely separated from the tibia, and the intervening gap was filled up by a large translucent fluctuating swelling which had gradually appeared since the last accident.

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